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THE MATHEMATICAL GAZETTE

EDITED BY
T. A. A. BROADBENT, M.A.

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VOL. XX. MAY, 1936. No. 238

WORK FOR UNIVERSITY ENTRANCE SCHOLARSHIPS.*

Mr. A. Robson (Marlborough): At the annual meeting a year ago Prof. Neville contrasted the Tripos and scholarship papers set at Cambridge in the last thirty years. Whereas he thought that he would cut a poor figure if he tried to take the Tripos of 1935 with his knowledge of 1909, he claimed that with his equipment of 1906 he would be able to pick up a scholarship in 1935. A perusal of the papers showed, he said, and we can accept his assurance, that whereas the Tripos had developed as one might expect an examination in a live subject to develop, the scholarship papers had remained stagnant.

Prof. Neville was careful not to assign blame either to schoolmasters or to college examiners, but he implied that those two classes of people might profitably take stock of the situation. That is what we are doing to-day.

It would be possible to take the line that although the Tripos papers contain questions involving many new developments, perhaps not many candidates are familiar with all those new subjects: 100 per cent. is not required for a First Class in the Tripos.

Also, in the scholarship work, there is a certain amount of elementary mathematics that was included in 1906 and still must be included to-day. It might be argued that there is not room for much outside these necessaries.

Although these two arguments have some weight, I would be sorry to rely on them alone.

I suggest that the teaching has *not* remained static. The biggest change has probably been in analysis, and I attribute that change largely to the influence of Prof. Hardy's book *Pure Mathematics*—partly also to the fact that, as time goes on, more and more teachers have learnt their analysis at Cambridge from lecturers who take the

* A discussion at the Annual Meeting of the Mathematical Association, January 3rd, 1936.

same line as that book. I imagine therefore that the average scholar probably goes up to Cambridge better equipped in general knowledge of elementary analysis than did his predecessor of thirty years ago. I do not think that it is important that he should know much about such things as the conditions under which he may differentiate or integrate a series term by term, multiply two series together, or decide whether a series which defies the easy tests is convergent. These things might be learnt in the last six months at school, but are they not better left for the university ?

The amount done at school is not identical with the amount that ought to be expected in the scholarship examinations ; chiefly because of the six months between the December of the scholarship examinations and the end of the school year in the following July. I hope that this point will not be overlooked in the discussion. It is a reason for not making considerable additions to the scholarship syllabus.

But there might perhaps be more questions in the scholarship papers to test general knowledge of elementary analysis, without going far into the developments. Here is a question of the kind :

Comment on the following statements :

(i) If θ is small, $\sin\theta$ is approximately equal to θ.

(ii) The sum to infinity of a series is the sum to n terms when n becomes infinite.

(iii) $8^{\frac{1}{3}}=2$.

(iv) An equation of the nth degree has n roots.

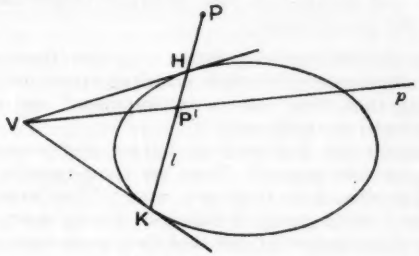
This actually comes from a scholarship paper, but such things might be asked more often. And it must have been about the beginning of the century that Prof. Hardy " asked a number of candidates, including several future Senior Wranglers, to sum the series $1+x+x^2+\ldots$ and did not receive a single reply that was not practically worthless ". What would happen if the experiment was tried to-day ? I like to think that the response would be better ; and if so, some progress must have been made in the last thirty years even in post-certificate teaching.

In geometry my impression is that progress in teaching has been less general in the schools. Here are three possible causes for this : first, geometry taught in 1900 was not absolutely wrong like much of the calculus, it was only rather narrow and excessively clumsy ; secondly, there is no book to do for geometry what Hardy's *Pure Mathematics* has done for analysis ; and thirdly, the influence of Cambridge has perhaps been smaller in geometry, because, until Prof. Baker's influence began to show itself, there was, if I am not mistaken, a scarcity of men whose interests were primarily geometrical.

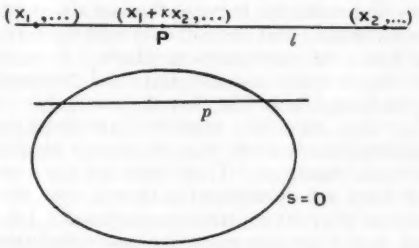
But is much change necessarily called for in the type of geometry question ? It may be new answers rather than new questions that are needed. Vectors, for instance, or modern projective methods such as those of Duporcq's book, help to answer questions of

geometry and not merely questions about vectors or projective methods. They do not necessarily call for special questions. Here is an example of a question that might well have been asked in 1905 and 1925:

"Why is the cross-ratio of four points (P) in a line l equal to the cross-ratio of their polars (p) with respect to a conic?"



(i)



(ii)

In 1905 probably most candidates would not have been ashamed to project the conic into a circle. If the question had been set last month I would hope that they might have said that in Fig. (i) $(PHP'K)$ is harmonic so that P, P' are in involution, hence $(P) = (P') = V(P')$. Or possibly in Fig. (ii) that the points $(x_1 + Kx_2, \ldots)$ have the same cross-ratio as the lines $s_1 + Ks_2 = 0$, each being the cross-ratio of the parameters K. For it is realised nowadays that there is not necessarily special merit in avoiding analytical methods.

Now the candidate who uses the first proof assumes without question that l meets the conic. He should be prepared therefore to face some cross-examination about the nature of that kind of geometry in which a line can be trusted to meet a conic. Also in the second proof some knowledge about the cross-ratio of parameters is assumed. As in analysis so here there seems to be room for some simple questions of general knowledge; *e.g.*, explain what is meant by a point at infinity.

Possibly some other small changes are desirable but the papers set a year ago in the two main groups of colleges seem well balanced in geometry. They contained :

> Three questions on very elementary geometry, two questions on reciprocation, six on projective geometry, two on homogeneous coordinates, and eight on cartesian equations (of which two involved parameters, two involved tangentials, and two were on the general conic).

Thus, I think, the old type of question on geometrical conics or the parabola and ellipse referred to their principal axes is not too much in evidence seeing that these curves *are* important and *do* illustrate general geometrical methods well.

Another speaker will deal with mechanics, and someone else will refer to the bookwork papers. These are the papers in which only about three questions have to be answered. They were introduced in 1905 in the Trinity group of colleges. I only want to say that they have my warm approval and that they were commended, soon after 1905, by the teaching committee of the Association.

I have also omitted to mention certain parts of algebra, *e.g.*, dummy suffixes and matrices, because the last six months at school seems the time for these. But some of us would welcome suggestions about algebra from the university speakers. A large section of algebra, the logarithmic, exponential and binomial (infinite) series, have practically been transferred to calculus. Some other items may have been dropped : continued fractions and the questions about equating coefficients of x^n in strange identities in order to prove other such identities. Then there are the questions about the number of ways a lady without a thumb (but with fingers of infinite length) can wear fifteen rings on one hand. I do not despise those questions, and I am sure examiners must find them useful for picking out good candidates, but they do not call for much teaching time. Is it better to devote some of that last six months to some modern algebra rather than to embark on elaborate analysis ?

Lastly, I hope that I have not given the impression of defending the schoolmasters at the expense of the college examiners, because I am very conscious that many improvements that we are able to introduce in school teaching are due to the admirably suggestive questions that occur from time to time in the scholarship papers, and especially in the bookwork papers.

Mr. G. L. Parsons (Merchant Taylors') : May I say at once that I agree with practically all that Mr. Robson has said, and if I do not reiterate his points, that is simply because I think that he has done them sufficient justice. In approaching the subject of scholarship work in mathematics, there is first of all one general observation I should like to make. It is not exactly relevant to the discussion, and yet I am sure that this discussion will not accurately represent the views of schoolmasters on this subject if I omit it. I mean that we must realise clearly that the problem of training university scholars

in mathematics is only a part, and a small part at that, of post-certificate mathematics as a whole, and that post-certificate mathematics is itself only a part of post-certificate education. There are not a few of us who feel that the two or more years after the school certificate are the two most vital years of the educational course, and that mathematics at this stage has something to offer to the boys who are not going to a university as well as to those who hope to do so. I often wonder whether our fellow-workers in universities have a clear idea how many groups of students there are who are occupied in post-certificate mathematics. I speak mainly for London schools, where you will find intending scientists, engineers, actuaries and mathematicians proper—with a few intending surveyors, army candidates and civil service candidates thrown in—not to mention the occasional boy who wants to " go on with maths." because he cannot think of anything else to do. Very few schools have enough men to cope with all these groups separately ; some, indeed, have only one man and he has to cope with all of them at the same time. This pressure on the teacher's time, variable though it is in different schools, is a factor which cannot be ignored at any stage of this discussion, if the discussion is to remain on the practical level. It means that in many cases the teacher is debarred from giving to the mathematical specialists (or specialist, in the singular) the attention he would like to give and the boys are driven back to textbooks. University lecturers as a class are dissatisfied with existing textbooks (they usually refer vaguely to a MS. in a drawer somewhere which will one day set the matter right) and a deadlock at once ensues.

There is a second general factor which should be noticed, especially at a meeting of this Association. The widespread propaganda of the Association has led to a great increase in the number of pupils who take " additional mathematics " in the school certificate examinations. In our own schooldays comparatively few boys (except intending specialists) learned trigonometry or mechanics at the pre-certificate level. Nowadays something like 30 or 40 per cent. of the candidates in many schools offer these subjects. Naturally this increase in numbers leads to a slowing down in pace and that, coupled with the slightly later age at which the school certificate is taken, means that in many cases the really brilliant mathematical boy does not sometimes get quite so far ahead by the time that the certificate is taken. Possibly I should say that he has not acquired nearly so much manipulative skill at this stage and that a good part of his first year of specialisation must be spent on consolidating his elementary work in algebra, trigonometry and so forth. But at the other end of the scale the opposite has been happening. I belong to the school of thought which considers that university mathematics has extended in scope, presumably owing to the " regression of the research line ", and that the amount of knowledge expected at the scholarship stage has, to a certain extent, followed suit—for, it is claimed, if boys do not know such and such things when they come up, how will they be ready to do such and such things when they

have finished their course. A colleague of mine has wittily referred to this process as "the pursuit of an ever-receding point with the object of making its rate of recession faster when you reach it". Thus it has come about that some senior members of this Association did not start calculus till after their scholarships, while I, in common with several of my contemporaries, knew no integral calculus at that stage. What would be the fate of the boy who knew no integral calculus now ?

Many of us feel the effect of this ever-widening gap between school certificate and scholarship, and let me say at once that we emphatically repudiate any suggestion that the right solution is to cease teaching more advanced mathematics to any but potential university scholars.

The situation would be considerably eased if the scholarship syllabus had discarded as fast as it has acquired—true, continued fractions and the theory of numbers have gone, but much of the old analytical trigonometry still remains and many statics questions are so overloaded with algebra and elimination that practically no statics is left. We still have at times to endure the antics of the lunatic who attempts to pile four billiard balls in a pyramid at the bottom of a hemispherical pudding basin, but really illuminating pieces of statics, such as bending moments and indeterminate cases of equilibrium (after all, many of the simplest things we meet are indeterminate cases), seldom find a place. Partial differentiation might well wait till later ; perhaps it might come in that six months when Mr. Robson hopes to do so much but when I, personally, feel that I shall be fully occupied looking after next year's batch ! I have always felt, too, though not all of you will agree, that our analytical geometry tends to follow up the equation of the second degree to too great a degree of elaboration.

I have no wish to appear as an accuser, but it is difficult to maintain, after looking through several volumes of scholarship papers, that manipulative skill rather than understanding is not the object of many of the questions set. Selection according to manipulative skill is a hit-or-miss method which tests the teacher's powers of foresight to a far greater extent than the pupil's mathematical knowledge. Many people feel, too, that it is not even the best way of selecting potential research workers. Let no one take it that I admit this to be the only object of electing mathematical scholars ; but even if it were, is not an over-insistence on such mechanical work calculated to produce the type of pupil who suffers all through his course from mathematical dyspepsia ? That is why I lament the passing of the essay and bookwork type of question to which Mr. Robson has already referred. I am told that it was dropped because it lent itself to "cramming". I am not sure that the argument is valid, for I do not think there would be the least difficulty in distinguishing between the work of the boy who had merely learnt the textbook by heart and that of the boy who showed some originality and understanding in presenting the matter. And what, after all, is "*cramming*" ? If

it consists in teaching certain things rather thoroughly because they are more important and fundamental than other things, is not this exactly what our employers pay us to do, and why exactly must we be circumvented when we do it? I believe, in fact, that these questions of the essay type were dropped, not because they lend themselves to "cramming" any more than stock examples do, but because "cramming" becomes so much more transparent in them—surely an argument for their retention. In almost every other subject the scholarship candidate has a fairly clear idea of some of the work he will be called upon to do; the classic knows that he must versify in a certain set manner, the historian that his knowledge of certain vital junctures in history will be tested, but the mathematician just does examples. I do not see how it is to be wondered at that, in spite of all the emphasis placed by teachers on basic principles, examiners in such an examination always complain that candidates appear to have no grasp of them. I put forward a strong plea for more and better essay questions and bookwork papers.

Coming to the questions themselves, it seems to me that the papers show evidence of insufficient revision, especially in relation to the even covering of the syllabus. I recall one mechanics paper of about ten years ago in which three of six dynamics questions were on various aspects of projectile motion. It is admitted that it must be extremely difficult to cover so large a syllabus in so short an examination, but we should all feel happy to know that great attention is paid to this point.

I have spoken much of generalities and perhaps you are disappointed. I defend myself because I am sure that it is on general ideas and not on particular points that we differ most, as, of course, is natural. It is difficult to point to a particular question or topic of which one can say that it should not be set or should never occur, and yet it is not always easy to believe that the aggregate of the topics and questions which do occur is ideally suited to the purpose it is intended to fulfil. This difficulty we know that you, our coworkers for the universities, feel, and we are glad that you feel it sufficiently to come and talk it over with us. We shall not exhaust the subject, I am sure, but we do await your contribution to it with the liveliest interest.

Mr. M. H. A. Newman (St. John's College, Cambridge): I hope I may be excused if my contribution to this discussion appears to ignore many of the practical problems that confront teachers who are preparing boys for university scholarships. It is not from a failure to realise how serious these problems are, but because I feel unqualified to discuss them, that I have passed them over; and because a discussion of what would be the proper syllabus if only work for the university had to be considered is at any rate a step towards deciding what should be taught in the real, more complicated, circumstances.

If all practical difficulties are left on one side, one of the main problems remaining is where to strike the balance between imparting

facts and manipulative skill, on the one hand, and, on the other, looking after the student's general mathematical education and background. This general education may, perhaps, best be defined as teaching the boy what mathematics is really like, and an important part of it is making him realise how manipulative technique is blended with the more abstract and theoretical side of the subject. The familiar phenomenon of the boy who does well in the scholarship examination but comes to nothing at the university is often due, not to lack of general intelligence in the boy, but to his having mistaken what it is that he is really interested in. He thought it was mathematics, but it was really something else.

There is little danger nowadays that the theoretical side will be understressed—indeed, there may be a risk of the reaction against pure formalism going so far that technique is held up as a purely disagreeable necessity, of no value or interest in itself. I do not share this view: the necessary symbolical work, which lies at the heart of most proofs, must be done with gusto if it is to be done at all. However, this occasional tendency among the higher practitioners is not the danger that is likely to trouble the schoolboy. His danger is the opposite one: that having taken pleasure in his skill at calculation and the manipulation of symbols, he will go up to the university and there be increasingly bewildered and disheartened by the large amount of argumentation about colourless abstractions that confronts him. It is important, then, to give him an early opportunity of forming a reasonably accurate idea of the nature of mathematical studies. An obvious general principle is that each large division of the subject should be presented, as far as possible, as a unified theory, moving steadily on, without isolated fragments that have no apparent relation to the whole. I am thinking here not only of such genuine blind alleys as the detailed trigonometry of the triangle (properties of r and R, etc.), but also of pieces of work quite respectable in themselves which are made to appear meaningless by a premature appearance, detached from their context, because they happen to be capable of elementary treatment (and are perhaps fertile in examples). An example is the solution in integers of indeterminate equations.

The actual make-up of the syllabus brings us to questions related to the subjects of several recent discussions and addresses to the Association. It is a problem closely related both to Professor Neville's presidential address last year and to Mr. Dockeray's most interesting article in the December number of the *Gazette*. All the speakers in these discussions seemed prepared to assume, almost as a matter of course, that the proper medium for introducing the general theoretical aspect of mathematics is analysis, more or less rigorously treated, but I am very doubtful if that assumption is correct.

There are several stages between the pre-Hardy differential calculus and the full-blown modern Theory of Functions of a Real Variable. An obvious intermediate step is the combination of *rigorous* definitions of the analytical concepts, such as limit, continuity, etc., with a *naïve* account of the real numbers, and of those

theorems that need Dedekind's theorem for their proof. But another important stage is more easily overlooked, namely the treatment in which the most fundamental concepts, limit, convergence of sequence, continuity, differential coefficient, etc., are explained fully, with some specimen theorems and their proofs, but without any attempt at a complete development of the theory on this level, with proofs of all the necessary theorems.

The difference between these two stages—the systematic and the unsystematic treatment—is, I think, a very definite one. Most students take readily to the *idea* of arithmetical definitions of everything, and the clearing away of clouds that they bring with them, and they admire the project of setting up the whole picture, but if they are immediately plunged into the long succession of theorems, all, to them at any rate, rather alike, by which the programme is actually carried out, they grow disheartened and their original interest is in danger of being extinguished.

It is surely agreed by all teachers of mathematics that in learning a mathematical theory a period of repose is necessary after each considerable step, in which what has been learnt can, somehow, settle down; in which it can be looked at from all sides, its consequences tried out, and its relation to our other knowledge appreciated. This is true not only of schoolboys and undergraduates, but of anyone trying to learn a theory that is really new to him—at least that is my own experience. Such a period of repose I should expect to be especially necessary between the two stages I have mentioned: between learning the first few fundamental arithmetical definitions and starting out on a systematic exposition of the theory of functions. In that period the pupil would be making himself at home with the definitions by specimen theorems, by examples, and, above all, by geometrical illustrations. The one serious gap which I find in the equipment of a certain number of entrance scholars is that when confronted with some difficult new concept or theorem, whether it is the upper limit of a sequence, or the change of variables in a double integral, or a nowhere differentiable function, or an existence theorem for differential equations, they do not at once and instinctively draw a picture. Mr. Dockeray in his article drew a gloomy picture of a university lecturer wondering what the boys *are* taught at school. Such university lecturers must be very rare. I myself have found scholars, and even near scholars, to be absolutely sound on those basic facts and pieces of technique which must be assumed as a groundwork for university teaching. But I do sometimes find a tendency to think that the arithmetisation of analysis means that it is no longer permissible to draw a picture to find out what is behind an arithmetical result. This is a very serious handicap, and one from which a certain number never recover. I can think of nothing more important than the implanting of a sound idea of how to use geometrical intuition in following or constructing an arithmetical proof. But the explanation must surely be a leisurely process. It might proceed by a very full discussion of certain early theorems, and part

of it might perhaps be hung on to examples of queer functions, which catch the imagination and make a good text to preach from. This careful but discursive and unsystematic treatment of the basic ideas of analysis would, in my opinion, form the ideal foundation on which to build up the systematic university course.

The outcome of all this seems to be that analysis is *not* suitable for a really uncompromisingly rigorous development in the school course : it is too hard. It still remains, then, to find a subject which will provide an early example of strict mathematical argument, using its stated assumptions and no others. I believe this is to be found in a subject which is at present totally neglected in comparison with analysis—I mean algebra, particularly linear algebra, the theory of linear dependence, matrices and so on ; and a little of axiomatic algebra. It seems to have everything to recommend it for a place in the syllabus at this stage. It is, like analysis, a living subject which leads straight up to work now being done, and it is useful in every part of modern mathematics. Its ideas are much simpler than those of analysis, to which it is in every sense antecedent. It has the right sort of mixture of theory and example. Above all, it has not the puzzling antithesis between " rigorous proofs " and " intuitive or natural proofs " which gives analysis its peculiar difficulty for a large number of students. It is a great pity that the first example they meet of strict mathematical reasoning should lead them to suppose that rigour is something which is stuck on to a proof, which consists in finding difficulties where none was suspected before, in order to dispose of them again by a very difficult argument. The difficulty must be met sooner or later, but it need not be their first and, for a time, their only idea of what rigorous mathematics is like. Now an axiomatic treatment of, say, the complex numbers should have the opposite effect, by removing the mystery which clings to $\sqrt{(-1)}$, especially if a glimpse is given of the possibility of arriving, by similar methods, at still more general number systems, among which the real and complex numbers take their place. The geometrical explanation of $\sqrt{(-1)}$ or the *ad hoc* definition of complex numbers as real " number pairs " does not quite dispose of the queer fact that $\sqrt{(-1)}$ appears naturally as a genuine number after it has apparently been shown not to exist. The whole of the mystery disappears if it is pointed out that the numbers which cannot have squares equal to -1 are based on a different set of postulates from those which can.

I would not, however, suggest that more than a very little should be said on these axiomatic matters, to give salt to the course. What I am thinking of mainly is a perfectly straightforward course on the rank of matrices, general simultaneous linear equations, etc., with considerable emphasis on the idea of linear dependence, and its geometrical counterpart, the vector, that underlies them all. Such a course would, I believe, provide an attractive sample of a unified mathematical theory, without the excessive difficulties that attend a strict presentation of analysis.

How it is to be fitted in is the inevitable question. In general terms I think it clear that it should be at the expense of analysis, even if it should mean a lowering of the standard of what knowledge of that subject can be assumed in the entrance scholar; and if it is agreed in principle that the balance between the two subjects in early teaching needs readjustment, the method of carrying the change into effect may be classified as one of those practical problems which I announced at the beginning it was my intention to shirk.

Mr. A. H. Wilson (Trinity College, Cambridge): Although the main purpose of the scholarship examination is to enable colleges to choose scholars, some of whom turn out to be good and others bad investments, the examination ought to be arranged so as to ensure, as far as possible, that any scholar is adequately prepared for starting his university career. I see no prospect of devising a syllabus which will always distinguish the geese from the swans, when the geese have been dressed up for the occasion, but something can be done to save boys from finding themselves hopelessly lost from almost their first day at the university.

It might be helpful to point out some of the difficulties which arise in applied mathematics at the university. Mechanics is the only scholarship subject in applied mathematics, and in the examination it is restricted almost entirely to statics and particle dynamics involving one degree of freedom. Many boys who have a good knowledge of elementary dynamics lose their grasp of the subject as soon as they reach problems dealing with two and three degrees of freedom. I do not see how this can be altogether avoided, since dynamics is of quite a different order of difficulty at this stage from what it is at scholarship standard, and on the whole I think the mechanics paper is quite a fair test of a schoolboy's knowledge. I would not wish it to be made harder, and we shall have to continue to risk finding a student stumped in the middle of his first year. A greater insistence on the principles which render problems soluble, namely the conservation of momentum and the conservation of energy, would, however, do something towards making rigid dynamics less formidable of approach. Most students' ideas concerning momentum are extremely hazy, and it is very difficult to get them to realise that Newton's laws are not a mere set of rather incomprehensible rules for solving examination questions, but have a very definite physical meaning and even an experimental basis. Problems involving variable mass are usually set with the view to drawing attention to the importance of the momentum rather than of the acceleration, but I doubt whether they do much good. Many of the other questions are set with the same end in view. Unfortunately problems involving one degree of freedom can be easily solved in so many different ways that it is impossible to emphasise the principles of energy and momentum as much as one would like. I need scarcely insist on the desirability of employing elementary calculus from the very beginning of dynamics. If a boy cannot appreciate the gradient of a curve, it is unlikely that he will understand what is meant by saying that the

rate of change of momentum is equal to the force. Further, in considering alternative proofs and methods, the best, in my opinion, are those which extend to more general problems.

Although mechanics is the sole representative of applied mathematics in the scholarship examination, it forms only about one-third of the applied mathematics in the Tripos, the remainder being electricity, hydrodynamics and thermodynamics. It is unreasonable to expect anyone to follow a university course on electricity without a very considerable background of physical knowledge, but nothing is done to ensure that this background exists. Rather the opposite. It is only natural to concentrate at school on scholarship subjects, and yet this concentration often means the subsequent downfall of a scholar, because his education has been too narrow. Electricity and magnetism—also hydrodynamics and thermodynamics even more so, but I restrict my remarks to electricity—is essentially a physical subject, and no amount of mathematical technique can replace physical insight, which can only be obtained by a practical familiarity over a long period with the problems which arise. I firmly believe that all applied mathematics—mechanics included—should be taught with a large bias towards the practical side, since otherwise the subject rapidly becomes academic and useless—as, for example, the classical hydrodynamics of yesterday. More attention to laboratory work would eliminate the major entrance scholar—I quote an extreme case, but such do exist—who to the end of his days never knew whether it was the charge or the potential which became zero when a conductor was earthed. Failure to understand advanced electricity is nearly always due to inadequate physical knowledge, and it is a pity that the obvious and simple remedy is not more often applied.

At the moment some boys do come up well prepared in physics, but the majority do not, and it is obvious that the option of taking physics and mathematics together in the scholarship examination is not a sufficiently attractive one to induce most schools to spend much time on physics. It may be objected that the syllabus is already so full that it would be difficult to spare the time. To this I would answer that electricity is in many ways a more elementary subject than is dynamics, and that much valuable time which used to be spent on avoiding the use of calculus in dynamics might be better employed in imparting a good knowledge of elementary physical principles. Theoretical physics forms the largest and most important part of the university applied mathematics course, and I think this ought to be recognised in the scholarship examination. Personally, I should like to introduce a paper on elementary physics and to see the mechanics paper savouring rather more of applied mechanics than it does at present.

Dr. B. Swirles (Manchester University) supported the plea made by Mr. Parsons for more questions of the essay type in his remarks on the mechanics paper. Would he not only support that, but also questions not necessarily on the bookwork that candidates might be expected to have been taught, but on their general reading; on work

with which they might become acquainted either through the press or through the many fairly good popular expositions of the modern theory or history of mathematics?

Mr. W. Hope-Jones (Eton) said that he had been a little disappointed by the absence of any reference to probability; the almost complete absence of reference to trigonometry—what there had been had been mostly derogatory; the absence of any reference to spherical trigonometry, with its bearing on astronomy and geography; and the almost complete absence of reference to three-dimensional work. The first, and, he thought, one of the only two references to three dimensions, was that about the lunatic, four billiard balls and a pudding basin. He was a lunatic, and deeply attached to four billiard balls and a pudding basin; and he did not think it possible to improve a boy's education by cutting out things of that sort.

A certain amount had also been said about unifying mathematics by cutting out the odd jobs. Personally, he would put in a plea for odd jobs. Very often the little things called out a boy's imagination and stimulated him to think for himself, more than was likely when he travelled well-trodden roads. Of course they could not desert the main lines, but let them encourage a boy to do something of the theory of numbers and, to take a particular instance already mentioned, be able to give an integral solution of indeterminate equations. That was one of the things he would not choose for leaving out. It was by means of little things like that that a mathematician justified his mathematics to a world of human beings who were not mathematicians, and was able to keep his end up among classical masters, grandmothers, lower boys in his house and other people of that sort when they asked him how to do the mathematical puzzles that they found in the papers. He believed it to be a rottenly bad advertisement for mathematics in general if mathematicians were unable to do those things better than other people. Let them not reduce themselves to the level of the high-class musician who could play any perfectly unintelligible music but could not go through a simple hymn tune at the children's Sunday school.

Mr. H. F. Marks (King's School, Grantham) asked if the new division of the mathematical Tripos was likely to affect what was required from candidates going up for scholarships, now that Part III was separate and only two parts could be done. And was there any specific reason for the exclusion of hydrostatics from the applied mathematics paper? A number of schools did hydrostatics. Mr. A. S. Ramsey in his book on hydrostatics said that while his first two books were intended for schools and universities, his *Hydrostatics* would be of service only to those in their first year in universities, as hydrostatics was not normally taught in schools. The speaker expressed the opinion that quite a lot of schools did teach it.

Professor E. H. Neville (University of Reading) said that when a little more than a year ago he had discovered a subject on which to speak to the Association he had no idea whatever as to what would be the really popular topics in the English press as the year came to a

close, but somewhere appropriately in March or April there were stirrings in the background and that morning he claimed credit for having presented the Association with as fine a set of quadruplets as they could wish for, and there had been a bright amount of squabbling in the nursery.

He was particularly interested in Mr. Newman's suggestion that teachers must not be afraid to introduce some ideas and a simple proof or two while at the same time not proposing to go into the complete logical developments of a theory to which those ideas belonged. It seemed to him quite wrong that any student should leave the university, or even get to it, without a perfectly clear knowledge that the whole difficulty connected with points at infinity and their relation to accessible points disappeared completely when the name of point was transferred to a collection of lines satisfying one simple condition; that the change in vocabulary solved the whole problem, removed all the anomalies; that when it was stated that there was one, and only one, point at infinity on a line this was merely Playfair's axiom in another form. He saw no reason why the kind of student they were considering that morning should not appreciate that, should not receive the translation of a couple of ordinary theorems into the new language and understand it perfectly. That was not to say that anybody at school or elsewhere should be expected to spend the time necessary to go into the complete logical development of that theory. There were a large number of books which had to be written and need never be read. He was not sure whether *Principia Mathematica* was not one of those books. But it was quite easy to explain to pupils why those books had to be written.

He was, however, inclined to think that excursions like this probably belonged to the spare six months of which Mr. Robson had spoken; they were the sort of things usually indulged in when examinations were out of the way and one wanted an interesting week or two at the end of a term.

There was one point on which Mr. Parsons and Mr. Robson were making almost contradictory demands, because it was extremely difficult for an examiner to set a paper which was to cover the syllabus without evoking the criticism: "Yes, there will be one question on ellipses, one on the rectangular hyperbola, one on this and one on that." The ideal that one should cover the syllabus and yet not be able to state in advance exactly how the paper was to be constituted was unattainable, as any examiner who had tried to convince fellow-examiners that it was suitable on a particular occasion to have two hyperbolas and no ellipses would agree.

Mr. A. C. Heath (St. Paul's School) thanked Mr. Wilson for his remarks with regard to physics and the desirability of including it in the school course, and asked Mr. Wilson whether it was not possible that that should be more encouraged than now. It was possible for a boy to take physics as well as mathematics in his scholarship examination, and he often did so, but he usually went home with the tale that his tutor had told him that his physics would not do him any

good unless he was exceedingly good at it; that if he could reach £60 standard in physics and £60 standard in mathematics he only got a £60 award. That discouraged the man who had been teaching the physics, and played into the hands of the mathematics master.

The speaker also asked what was the right course for a boy to pursue who wanted to take up physics. At present in the scholarship examination he must offer physics and chemistry to a first-class standard and mathematics to a rather low subsidiary standard, in which practically everybody could get 80 per cent. marks without difficulty. Was there any hope of extension of the scheme introduced at one college only at Oxford whereby the physics scholar took physics and mathematics as his main subjects ?

Mr. A. H. Wilson said it was true that the physics examiners required a high standard. He could not say exactly the percentage they did require. A boy who was in the exhibition class and got into the second class in physics would probably be put into the £60 class in mathematics. He would not be put up into the £100 from the £60 class unless he obtained a first class in physics. That was a ruling made by the physics and not the mathematical examiners. As regards the examination in natural sciences, it would be difficult to raise the standard of the mathematical papers set. The scholarship examination in natural sciences was not only for physicists and chemists but also for those working in biology, and there was a strong pressure from biologists that mathematics should be cut down as far as possible.

Mr. Robson wished to say a word of encouragement to Mr. Hope-Jones as to some of his odd subjects. Mr. Hope-Jones had mentioned spherical trigonometry, the sort of thing a scholar would do in a few days and which did not call for very much time. He (Mr. Robson) had not meant to exclude that. Three-dimensional work he thought all were in favour of. He did not know as regards algebra and trigonometry exactly what Mr. Hope-Jones had in mind, but he thought he could satisfy both Mr. Hope-Jones and Mr. Newman on one point. Mr. Newman did not want Mr. Hope-Jones to get up in school one morning and say : " Now we are going to do indeterminate equations. What fun ! " But he did not think Mr. Newman would mind if he said in the last week of term : " Now we are going to have some fun about the theory of numbers", and in the course of that they would hope to get as far as the solution of simple congruences, in which case they would learn to solve those indeterminate equations. But they would do it as part of a rather bigger thing and not as an isolated topic. He could not say more except that personally he would welcome the ' bookwork ' papers, though there was the difficulty of setting them on the present basis. He thought it would be well to broaden the basis in the way Dr. Swirles had suggested.

The President expressed the thanks of all present to the four openers of the discussion, and particularly to Mr. Newman and Mr. Wilson for so kindly coming to give their views. The more schoolmasters and university teachers could teach one another, the better it would be for mathematics generally.

FRACTIONAL CALCULUS.

By W. Fabian.

1. Let $f(x)$ be a real function of a real variable x. The meanings of $\left(\frac{d}{dx}\right)^{-\lambda} f(x)$, when λ is a positive integer, a negative integer and zero, are well known. In the first case, $\left(\frac{d}{dx}\right)^{-\lambda} f(x)$ denotes the λth integral of $f(x)$ with respect to x, with an arbitrary lower limit of integration. In the second case, $\left(\frac{d}{dx}\right)^{-\lambda} f(x)$ stands for the $(-\lambda)$th differential coefficient of $f(x)$ with respect to x. Lastly, when $\lambda = 0$, $\left(\frac{d}{dx}\right)^{-\lambda} f(x)$ means $f(x)$. But what does $\left(\frac{d}{dx}\right)^{-\lambda} f(x)$ mean when λ is a fraction ?

This problem of finding a meaning for $\left(\frac{d}{dx}\right)^{-\lambda} f(x)$ when λ is a fraction has been taken up by many mathematicians, of whom the more important were Leibnitz,* Liouville† and Riemann.‡ Riemann took the idea of the Taylor series and arrived at the definition

$$\left(\frac{d}{dx}\right)^{-\lambda} f(x) = \frac{1}{\Gamma(\lambda+\gamma)} \left(\frac{d}{dx}\right)^{\gamma} \int_0^x (x-t)^{\lambda+\gamma-1} f(t)\, dt,$$

where γ is a positive integer or zero such that $\lambda+\gamma > 0$, by assuming an expansion of $f(x+h)$ in the form

$$f(x+h) = \sum_{n=-\infty}^{\infty} k_n \left(\frac{d}{dx}\right)^{n+\nu} f(x) \,.\, h^{n+\nu}, \quad [n+\nu = -\lambda],$$

with the condition that this series should become a Taylor series for integral values of λ. That this definition is a natural one has been confirmed from numerous considerations, of which a full account is given by Ferrar.§

2. We define a λth integral, or a $(-\lambda)$th differential coefficient, of $f(x)$ over an interval (a, x) by

$$D^{-\lambda}(l_a) f(x) = \frac{D^{\nu}}{\Gamma(\lambda+\gamma)} \int_a^x (x-t)^{\lambda+\gamma-1} f(t)\, dt,$$

where D stands for $\frac{d}{dx}$, and γ is the least integer greater than or equal to zero such that $\lambda+\gamma > 0$; $f(x)$ is bounded on the path of

* Leibnitz, *Opera, ed. Dutens*, 3, *Commercium Philos. et Math.*, p. 105 (1695).

† Liouville, *Journal de l'Ecole Polytechnic*, 13, pp. 1-186 (1832).

‡ Riemann, *Gesammelte Werke*, pp. 331-344 (1876).

§ Ferrar, *Proc. Edin. Math. Soc.*, 48, pp. 92-105 (1927).

integration, and is continuous on this path except possibly for a finite number of ordinary discontinuities.

In the present paper the principal value of $(x-t)^{\lambda+\gamma-1}$ in the definition of $D^{-\lambda}(l_a)f(x)$ is to be taken. When no ambiguity can arise, we shall write $f_\lambda(x)$ for $D^{-\lambda}(l_a)f(x)$. Unless otherwise stated, λ and a are to be taken as finite.

We have shown in a previous paper * that functions can be expanded in the generalised Taylor series which Riemann assumed as a basis for deriving a meaning of $D^{-\lambda}f(x)$. The following four index-laws * are obeyed by the fractional integral :

I. *If* $\lambda>0$ *and* $\mu>0$, *then*

$$D^{-\lambda}(l_a)D^{-\mu}(l_a)f(x)=D^{-\mu}(l_a)D^{-\lambda}(l_a)f(x)$$
$$=D^{-\lambda-\mu}(l_a)f(x).$$

II. *If* $f_\lambda(x)$ *and* $f_{\lambda+\gamma+m}(x)$ *exist, where* m *is any integer such that* $m+\gamma\geqslant 0$, *then* $f_\lambda(x)=D^{\gamma+m}f_{\lambda+\gamma+m}(x)$.

III. *Let* $D^m f(x)$ *be finite and continuous in* (a, b) *for all integral values of* m *greater than and equal to zero. Let* λ *and* μ *be any real numbers, and let*

$$f(a)=f'(a)=\ldots=f^{(\delta-2)}(a)=0$$

if $\mu\leqslant -1$, *where* δ *is the least integer greater than or equal to zero such that* $\mu+\delta>0$. *Then, in* (a, b),

$$D^{-\lambda}(l_a)D^{-\mu}(l_a)f(x)=D^{-\lambda-\mu}(l_a)f(x).$$

IV. *Let* $D^m f(x)$ *be finite and continuous in* (a, b) *for all integral values of* m *greater than and equal to zero. Let* λ *and* μ *be two real numbers, and* ρ *the least integer greater than or equal to zero such that* $\lambda+\rho>0$ *and* $\mu+\rho>0$.

If $\rho\geqslant 2$, *let* $f(a)=f'(a)=\ldots=f^{(\rho-2)}(a)=0$. *Then, in* (a, b),

$$D^{-\lambda}(l_a)D^{-\mu}(l_a)f(x)=D^{-\mu}(l_a)D^{-\lambda}(l_a)f(x)$$
$$=D^{-\lambda-\mu}(l_a)f(x).$$

3. We shall prove now some new theorems.

Theorem 1. *Let* $f(x)$ *be bounded, continuous and non-zero in* (b, c), *where* $b\leqslant a\leqslant c$. *Then, if* $\lambda>0$, $f_\lambda(x)$ *has no zeros in* (b, c) *except* a.

Proof. I. First consider the case when $a\leqslant x\leqslant c$.

By the first mean-value theorem we have, since

$$f_\lambda(x)=\frac{1}{\Gamma(\lambda)}\int_a^x (x-t)^{\lambda-1}f(t)\,dt,$$

$$f_\lambda(x)=\frac{(x-a)^\lambda}{\Gamma(\lambda+1)}f(\xi), \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1)$$

where ξ is some value of t in $(a\leqslant t\leqslant x\leqslant c)$. By hypothesis, $f(\xi)$ is not zero. Hence, by (1), $f_\lambda(x)$ is not zero in $(a<x\leqslant c)$. Clearly $f_\lambda(a)=0$.

* Fabian, *Philosophical Magazine*, ser. 7, vol. 20, pp. 781-789 (1935). The index-laws and the Riemann series are dealt with here.

II. To prove the theorem for the case $b \leqslant x \leqslant a$, we note that

$$f_\lambda(x) = \frac{1}{\Gamma(\lambda)} \int_a^x (x-t)^{\lambda-1} f(t)\, dt$$

$$= \frac{(-1)^\lambda}{\Gamma(\lambda)} \int_x^a (t-x)^{\lambda-1} f(t)\, dt.$$

Hence, by the first mean-value theorem,

$$f_\lambda(x) = \frac{(x-a)^\lambda}{\Gamma(\lambda+1)} f(\eta), \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (2)$$

where η is some value of t in $(b \leqslant x \leqslant t \leqslant a)$. $f(\eta)$ is not zero by hypothesis, so that, by (2), $f_\lambda(x)$ is not zero in $(b \leqslant x < a)$.

The proof is completed.

Theorem 2. *Let $D^m f(x)$ be finite and continuous in (b, c) for all integral values of m from 0 to γ, where $b \leqslant a \leqslant c$, and let $f^{(\gamma)}(x)$ have no zeros in (b, c). If $\lambda \leqslant 0$, let $f(a) = f'(a) = \ldots = f^{(\gamma-1)}(a) = 0$.*

Then $f_\lambda(x)$ has no zeros in (b, c) except a.

Proof. The case $\lambda > 0$ has been discussed in Theorem 1.

Consider the case $\lambda \leqslant 0$.

Integrating $f_\lambda(x)$ by parts γ times, we get, in (b, c),

$$f_\lambda(x) = \sum_{n=0}^{\gamma-1} \frac{f^{(n)}(a)}{\Gamma(\lambda+n+1)} (x-a)^{\lambda+n} + f_{\lambda+\gamma}^{(\gamma)}(x)$$

$$= f_{\lambda+\gamma}^{(\gamma)}(x),$$

since $f(a) = f'(a) = \ldots = f^{(\gamma-1)}(a) = 0$. By Theroem 1, $f_{\lambda+\gamma}^{(\gamma)}(x)$ has no zeros in (b, c) except a.

The theorem is proved.

Theorem 3. *Let $D^m f(x)$ be finite and continuous in (b, c) for all integral values of m greater than and equal to zero, where $b \leqslant a \leqslant c$. Let $f_{\lambda-\delta}(x)$, where λ is not an integer, be bounded, continuous and non-zero in (b, c), δ being the least integer greater than or equal to zero such that $\delta - \lambda > 0$.*

Then $f(x)$ has no zeros in (b, c) except a.

Proof. Integrating $f_\lambda(x)$ by parts γ times, we obtain, in (b, c),

$$f_\lambda(x) = \sum_{n=0}^{\gamma-1} \frac{f^{(n)}(a)}{\Gamma(\lambda+n+1)} (x-a)^{\lambda+n} + f_{\lambda+\gamma}^{(\gamma)}(x), \quad \ldots\ldots\ldots\ldots (1)$$

where $$\sum_{n=0}^{\gamma-1} \frac{f^{(n)}(a)}{\Gamma(\lambda+n+1)} (x-a)^{\lambda+n}$$

is to be omitted if $\gamma = 0$.

When $\lambda<0$, then $\delta=0$, so that, by hypothesis, $f_\lambda(x)$ is bounded in (b, c). Hence, by (1), since $f^{(\gamma)}_{\lambda+\gamma}(x)$ is bounded in (b, c),

$$f(a)=f'(a)=\ldots=f^{(\gamma-1)}(a)=0$$

if γ is not zero. Therefore

$$D^\lambda(l_a)f_\lambda(x)=f(x) \text{ in } (b, c).$$

Now, in (b, c),

$$D^s f_\lambda(x)=f_{\lambda-s}(x)$$

for all integral values of s from 0 to δ, and $f_{\lambda-s}(x)$ is finite and continuous in (b, c) for these values of s. Also, if $\lambda>0$,

$$f_\lambda(a)=f_{\lambda-1}(a)=\ldots=f_{\lambda-\delta+1}(a)=0,$$

since $\lambda-\delta+1>0$. By Theorem 2, which we now apply to $D^\lambda(l_a)f_\lambda(x)$, the conclusion follows.

Theorem 4. *Let $f(x)$ be bounded and negative in (a, g), with lower bound m for $|f(x)|$, and be equal to a constant K in (g, b), where $a<g<b$.*

If $\lambda>0$, then $f_\lambda(x)<0$ at all points in (g, b) such that

$$(K+m)(x-g)^\lambda<m(x-a)^\lambda.$$

Proof. In (g, b),

$$f_\lambda(x)=\frac{1}{\Gamma(\lambda)}\int_a^g(x-t)^{\lambda-1}f(t)\,dt+\frac{1}{\Gamma(\lambda)}\int_g^x(x-t)^{\lambda-1}K\,dt$$

$$=\frac{1}{\Gamma(\lambda)}\int_a^g(x-t)^{\lambda-1}f(t)\,dt+\frac{K(x-g)^\lambda}{\Gamma(\lambda+1)} \quad \ldots\ldots\ldots\ldots\ldots\ldots(1)$$

Now, in (g, b),

$$\left|\frac{1}{\Gamma(\lambda)}\int_a^g(x-t)^{\lambda-1}f(t)\,dt\right|=\frac{1}{\Gamma(\lambda)}\int_a^g(x-t)^{\lambda-1}|f(t)|\,dt$$

$$\geqslant\frac{m}{\Gamma(\lambda)}\int_a^g(x-t)^{\lambda-1}\,dt$$

$$=\frac{m}{\Gamma(\lambda+1)}\{(x-a)^\lambda-(x-g)^\lambda\}$$

$$>\frac{K(x-g)^\lambda}{\Gamma(\lambda+1)} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(2)$$

at those points where

$$(K+m)(x-g)^\lambda<m(x-a)^\lambda.$$

By (1) and (2) the conclusion follows.

Theorem 5. *Let $f(x)$ be bounded and positive in (a, g), with lower bound m, and be equal to a constant K in (g, b), where $a<g<b$.*

If $\lambda>0$, then $f_\lambda(x)>0$ at all points in (g, b) such that

$$(m-K)(x-g)^\lambda<m(x-a)^\lambda.$$

Proof. In (g, b),

$$f_\lambda(x) = \frac{1}{\Gamma(\lambda)}\int_a^g (x-t)^{\lambda-1} f(t)\,dt + \frac{K(x-g)^\lambda}{\Gamma(\lambda+1)}$$

$$\geqslant \frac{m}{\Gamma(\lambda)}\int_a^g (x-t)^{\lambda-1}\,dt + \frac{K(x-g)^\lambda}{\Gamma(\lambda+1)}$$

$$= \frac{m}{\Gamma(\lambda+1)}\{(x-a)^\lambda - (x-g)^\lambda\} + \frac{K(x-g)^\lambda}{\Gamma(\lambda+1)}$$

$$> 0$$

at those points where

$$(m-K)(x-g)^\lambda < m(x-a)^\lambda. \qquad \text{W. F.}$$

GLEANINGS FAR AND NEAR.

1045. A WEIGHTY MATTER.

On the morning of the half-term whole holiday I reposed in a corner seat of the 9 o'clock train which was to take me away from boys and mathematics for a few hours. I was puffing tranquilly at my pipe and idly planning the day, when my peace was completely shattered by a loud and assertive feminine voice. It was an unmistakably "school voice", and a change of compartment began to appear advisable, but my resentment was changed to interest by the unfolding of the following conversation.

1*st Schoolmistress.* "She (a pupil) said that a piece of iron and a feather dropped together from a window should reach the ground at the same time."

2*nd S. M.* "Why! they wouldn't even reach the ground at the same place!"

1*st S. M.* "Yes, I know, I've read the subject and know all about it, but she would insist."

2*nd S. M.* "How annoying!"

1*st S. M.* "As I told her, it is a question of density; if she could get feathers and iron of the same density, then perhaps she would be right. They will mix up volume and density."

2*nd. S. M.* "Ah well! One knows a lot at eighteen!"

1*st S. M.* "Yes, as some clever man once said, 'None of us is infallible, not even the youngest'." [Per Mr. C. E. Kemp.]

1046.

If truthe maie trie it selfe
By Reasons prudent skyll,
If reason maie prevayle by right
And rule the rage of will,
I dare the trial byde
For truthe that I pretende,
And though some lyst at me repine,
Juste truthe shall me defende.

Robert Recorde, *Geometry—The Pathway to Knowledge* (1551). [Per Mr. J. Travers.]

RIDER-WORK IN GEOMETRY.*

Mr. E. H. Lockwood (Felsted): I hope it will be regarded as a platitude if I say that the only true test of an education is to enquire whether the mind is more active and capable at the end of the process than at the beginning. Those of us who have observed the bright, eager and imaginative children who continually enter our secondary schools and the dismal procession of dull, bored and jaded pupils who leave them will probably agree that as far as making the mind more active is concerned, the hope is rather a faint one. We can make the mind more capable, by giving it knowledge of facts, and experience of the various processes of thought. But the problem is, how to do that without losing on the side of activity and imagination as much as we gain on the side of knowledge and experience. If, as too often happens, we damp down the activity of the mind, the result is not only a general dullness, but more particularly, in geometry, a lack of capacity for imagination and intuition. By imagination in geometry I mean both the power to visualise figures and variations in figures, and a readiness to adopt new lines of thought; by intuition, the capacity to see what is true before a logical demonstration has been given.

In teaching boys to reason we have constantly to check their tendency to jump to conclusions, to rely on guessing and to generalise from experiment. It is very necessary to do so, but we must also remember that reason by herself is sometimes a lame walker and the ability to jump is not altogether to be despised. It is well known that intuition plays an important part in mathematical discovery, and probably it is the interplay between intuition and reason, as much as anything else, that provides the real zest of original work.

The question is, how can we give boys the exhilaration of using the full powers of their minds instead of the cramped feeling induced by the idea that of all the processes of thought deduction is the only respectable one?

Now if there is any part of school mathematics in which the mind is free to use all its resources, it is in rider-work in geometry: the successful solver must be able to explore his figure with an observant and roving eye, must be able to imagine how it may be varied, must spot relationships, must jump instinctively for the right construction, must argue backwards as well as forwards, before finally arranging his ideas in logical sequence. In America, riders are called "originals", and for the average boy they are certainly the nearest possible approach to original work. A very restricted kind of original work, perhaps, but on that account sufficiently easy for the average boy to tackle. Rider solving is an art; that is to say, an activity free within certain well-defined limits. The painter restricts himself to two dimensions, and perhaps further, to a particular range of colours, thus simplifying his task. His choice of subject is another

* A discussion at the Annual Meeting of the Mathematical Association, January 3rd, 1936.

restriction setting limits to the freedom of his work. My point is that these restrictions make the work easier. Given the necessary technique it is easier to paint the landscape in front of us than to paint a picture of nothing in particular. Most of us would find it easier to design a costume for Henry VIII or for Anne Boleyn than just to design a costume ; and in the same way it is easier for a boy to begin original thinking in geometry if he is told at first rather definitely what to aim at, as in the traditional form of rider.

But there is no need for him always to be guided to exactly that extent. The same rider may be presented in a variety of forms, and I suggest that we ought always to give preference, when it is practicable, to those forms in which the thing to be proved has first to be discovered. Take, for example, the very easy rider that the bisectors of two adjacent supplementary angles are at right angles. How much more interesting it becomes if presented in the form :

"Draw two adjacent supplementary angles, measure the angle between their bisectors and prove theoretically what the result should be." Or thus :

"Let AOB and OC be straight lines, and let OP, OQ bisect angles AOC, COB. If angle POC is 20°, calculate angle COQ. Do you suspect any simple relation between these two angles ? If so, try to prove it."

And afterwards, as a supplementary question : "If OC rotates clockwise about O, what happens to OP and OQ and angle POQ ?"

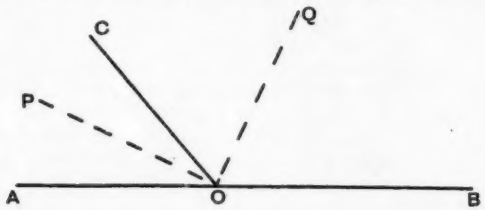


FIG. 1.

Or consider another well-known rider : "A circle cuts one straight line at A, B, another at A', B'. Another circle cuts the first line at B, C, the second at B', C'. And so on."

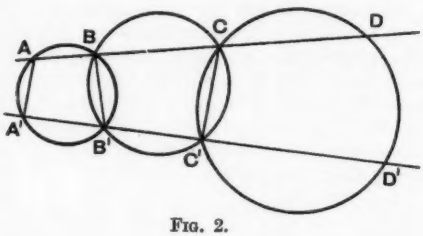


FIG. 2.

Why should we spoil all the fun by saying "Prove CC' parallel to AA'"? Why not say "Discover and prove anything you can about the chords AA', BB', etc."? It is a particularly good rider if stated in this form, because some of the class, if they have placed their first circle centrally between the two lines, may think that BB' is parallel to AA', a mistake that can be corrected either by imagining a variation in the figure or by deduction.

Or again: "If OT touches a circle at T, and OAB is a secant cutting it at A and B, and the bisector of angle O meets TA, TB at X, Y, find out what you can about X and Y and prove it."

Why should we deny our pupils the pleasure of discovering, if only by accurate drawing, that TX is equal to TY, and perhaps the excitement of wondering whether it is a fluke, and the interest of using deduction, their new tool, to show that it isn't.

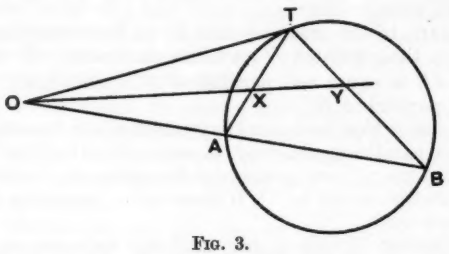


FIG. 3.

Of course it is not always necessary to draw the figure accurately. Calculations, arithmetical or algebraical, are equally helpful; and sometimes a relation can be spotted from a sketch figure, or a series of sketch figures.

For clever boys it is possible to do more ambitious original work. I know a boy of 14½ who discovered for himself (though he could not at first prove it) a property of polyhedra analogous to the fact that the sum of the exterior angles of a polygon is 360°. He then proceeded to use it to prove that there are only fifteen types of semi-regular polyhedra. That is a long way from ordinary rider-work, but perhaps we might begin bridging the gap by emphasising the "discovery" side of the rider-work.

I hope I am expressing the opinion of the Association when I say that the place of rider-work in geometry should be a very prominent one. It is sometimes said that riders should occupy about one-third of the time devoted to the subject, and I should like it to be added that much of the time given to theorems should be used in precisely the same way as the time actually labelled "for rider-work". You, sir, have shown us how to teach theorems on those lines and there is no need for me to say any more about it.

Mr. F. J. Tongue (Kingswood): Most of my remarks arise from my brief experience in teaching boys of only moderate ability in second mathematical sets. I feel that with able boys almost any

logically sound method of teaching will have good results. But with weak boys our task is much harder. I have tried the "formal" method of teaching the subject: great emphasis on theorems, definitions and proofs of theorems; I have found it unsuccessful with weaker boys. In the first place, it has no attraction for them and, in the second place, no time was found for any extensive rider-work.

I base what I have to say on two facts, which I think will not be disputed:

(1) Most riders in geometry books, set without a figure or hints, and depending on more than one theorem, are too difficult for the moderate boys.

(2) In School Certificate examinations riders are consistently avoided in favour of arithmetic or algebra or the writing out of theorems. This indicates a feeling that riders are the hardest questions in examinations.

With regard to the second point, I feel that questions are often set in a way that irritates or mystifies the reader. Two examples:

(i) "If AB is equal and parallel to DC, prove that AD is equal and parallel to BC.

My criticism is that we cannot say whether any theorem or known result about parallelograms may be assumed. If so, the question is done in two lines; if not, congruent triangles, etc., have to be used. A better question would be: "*Without* using properties of parallelograms, prove that . . ."

It is fundamentally important that in any rider we set, whether in class or in examinations, every child should *know* what he may assume. If there is any doubt in the teacher's or examiner's mind he should *tell* the child what he may or may not assume. For our purpose is not to find out the child's ignorance, but his knowledge. The same objection applies to many questions which are set as riders but which are really the theorem just proved stated in a slightly different way. The child thinks you are trying to "catch" him.

My second example illustrates other points:

(ii) "State without proof *two* angle properties of circles, unconnected with tangents or quadrilaterals. O is the centre of a circle QRS and T a point within the circle. A second circle passes through O and T and intersects the first circle at R and S, R being the nearer to T. OT is produced to meet the first circle at Q and TS, QS and RS are joined. Prove angle QRS equals angle QST."

Here the rider depends for success on a figure in which lines do not tend to coincide, and I defy the average boy to produce such a figure in two attempts! Even then a construction line has to be drawn—namely, OR.

Why could not either the figure be given, or else the construction? I submit that in both cases the questions are asking to be ignored by the candidate.

You may think that I am straying from my topic, which is not the

criticism of examination papers but rider-work in the classroom. I hope you will see that really my remarks are relevant.

For the mistakes made by the examiners in setting these questions are mistakes we all make in presenting the subject. They are: (*a*) ambiguity of what may be assumed; (*b*) the extreme difficulty in many riders of getting a figure before any start can be made in the proof; and (*c*) the difficulty of finding the construction in an already hard question. I do not suggest abolishing the difficult rider, but I ask for a fairer proportion of questions made simpler for the weaker boy. These mistakes comprise some of the reasons why we leave the weak boys struggling in the rear; and sometimes, if they are lucky, getting one rider done in a forty-five minute period!

I wish, in the remaining time at my disposal, to suggest a few ways in which we may help to enrich the subject for the weaker members of our classes, and to plead that something corresponding to this method of teaching may find its way into the textbooks: yea, and even into the public examinations! First, a few general notes on our aims and methods in teaching:

1. We should lessen the traditional emphasis on the learning of proofs. (Incidentally, why should such proofs be asked for in examinations? They are an invitation to cram.) Our object is to develop *power* in geometry. Symmetry and similarity should be freely used.

2. We should accordingly concentrate rather on the interrelation of theorems. We should classify them in different ways, so as to increase the boys' rapidity in drawing on them. I am stressing what Prof. Neville has called the "organisation" of elementary theorems and aspects of geometry.

3. We should familiarise our pupils with certain configurations of lines and points—these will suggest the relevant theorems and give them greater power in rider-work. Both of these last two suggestions will help to increase the proportion of rider-work that we can do. Examples of such configurations are:

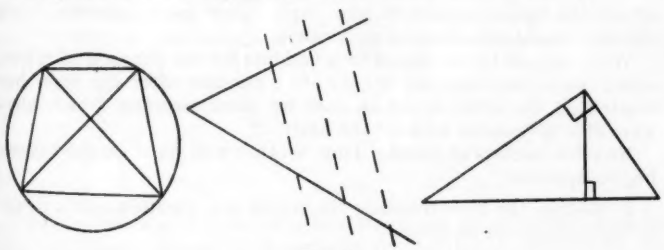


FIG. 4.

4. Geometry is to be tackled in an inductive manner. Everything is to be discovered by the boys themselves—they will then desire to prove the truth of their discoveries. Here, most naturally, accurate

drawing enters. There are many results that are suggested by a carefully drawn figure. In asking a class to draw a certain figure carefully and to look for interesting results we lead them to investigate the possibility of proofs. I call this the inductive method.

Let me quote some specimen riders tackled in this way:

(i) Investigate the figure formed by the angle bisectors of (*a*) a parallelogram, (*b*) an isosceles trapezium. The class will draw their figures. Some will obtain special cases for (*a*), *e.g.* a square or simply a point, but the majority will have an oblong. They will demand a proof, which can quickly be discovered. Similarly with case (*b*) a kite is obtained. Note the simple proof by symmetry.

(ii) The figure formed by the joins of the mid-points of the sides of a quadrilateral can be investigated. The special cases of a rectangle and a square will arise and will be examined theoretically. It is absorbing work and can be tackled by the weakest boy.

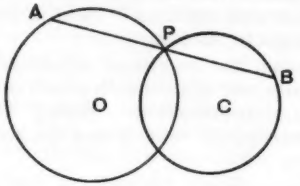


FIG. 5.

(iii) When is APB a maximum? What relation does it then have to other lengths in the figure? The examination of this question and its theoretical proof can be made by all the boys in the class. Other examples are: the concurrence and collinearity properties of the triangle, Simson's Line, etc. This method of doing riders leads to the excellent habit of jumping at possible properties of a figure and then checking such assumptions.

5. My last general point is that many riders must be done in which the figure is *given* to the pupil. Any good geometry book should contain selections of such riders.

We must not be too proud to give hints for the solution of riders, either when they are set or, say, five minutes after the class has started on the rider, in which case we must ascertain which boys were able to proceed without the hint.

Now for matters of detail. Time is often well spent on the following occupations:

1. Making the boys translate the words of a question into a figure in which data are marked.

2. Grouping theorems into various sets, each group containing theorems relating to a particular result—*e.g.*, to prove two angles equal, or two lines equal, or two lines parallel, etc.

3. Making the boys visualise the figures which belong to groups

of theorems. They should remember the figures rather than the enunciations.

My final plea is that the inductive method of tackling riders (closely akin to Mr. Lockwood's " discovery " rider) should find a larger place in the textbooks and the examinations. The living aspect of mathematics is its " inductive " one, and we must encourage that in all possible ways. Two such ways are to stress it in the textbooks and to require its use in examinations.

Mr. F. C. Boon (Dulwich) heartily welcomed the remarks asking for more induction.

He had for the last twelve years spent more than half his teaching time with classes of pupils somewhat under average intelligence, one class in particular having the very lowest intelligence in the school. The things he had found most difficult to teach were problems in algebra and riders in geometry. And he was inclined to think that with beginners it was more difficult to teach the former than the latter ; he thought that no one need despair of getting pupils of average intelligence, at any rate, to produce a good crop of riders, largely in the way suggested by Mr. Lockwood and Mr. Tongue.

As a corollary to what Mr. Siddons said three years ago, when he advocated plenty of practice in riders as a method of learning a proposition, the speaker believed that the method of learning propositions could be of such a nature as to help pupils to solve riders. It was essential, of course, to begin by getting one's pupils to recognise exactly what the question said, and especially to be clear as to what the definitions said. The speaker illustrated his contention by taking the first proposition about the parallelogram. There would first be discussion as to what the word " parallelogram " meant. It said nothing about equality. It meant a " parallel drawing " ; it was constructed by drawing parallel lines. It should not be drawn as in Fig. 6 (i), the chalk proceeding continuously from A to B and then to C, D and back to A. It should be drawn as in Fig. 6 (ii), by drawing two pairs of parallel lines.

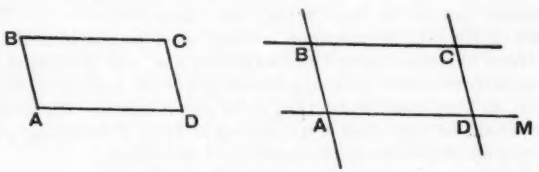


FIG. 6.

The pupil would then be asked to review what he knew about parallel lines. Using his knowledge on the figure, starting at any angle, say $\angle BAD$, he would have $\angle BAD = \angle CDM$ from one pair of parallel lines and $\angle BCD = \angle CDM$ from the other ; thus obtaining the fact that the opposite angles BAD and BCD are equal without using the proof given in his book.

If the pupil wished to prove what he perceives is true, namely that the opposite sides are equal, then it becomes a case of congruent

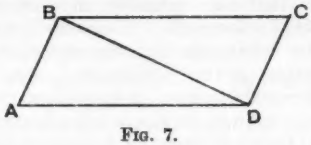


FIG. 7.

triangles. In writing out the theorem he should write not "Given the parallelogram $ABCD$" but:

"Given $AB \parallel DC$ (i)

$AD \parallel BC$ (ii)

then in the $\triangle$s ABD, CDB

$\angle ABD = \angle CDB$ ($\because AB \parallel DC$)

$\angle ADB = \angle CBD$ ($\because AD \parallel BC$)"

and so on.

In this way the pupil is helped to realise that he has used all the data—facts (i) and (ii)—and that these are necessary and sufficient to obtain deductions.

The speaker feared that this might seem rather simple and trite, but he also feared that such a method was not followed as much as it might be.

Mr. W. F. Bushell (Birkenhead) said the meeting had listened to two excellent papers and members were, he felt sure, grateful to the Association for bringing forward a subject of obvious interest. There was only one point of criticism he might urge with regard to the papers. He had much hoped to hear rather more as to precisely what was our aim in doing elementary rider-work. He supposed there were two or three different things they might be aiming at. Were they aiming, for instance, at the idea of definitely getting a solution, or were they more definitely aiming at the idea of making every boy put down, logically and accurately, step by step, in the usual form, the various points as they arose, one after the other? The old teachers of Euclid, for instance, always used to tell him that what they valued in Euclid more than anything else was the way it taught boys logically to develop an argument, point by point, on the sheet of paper, as they used to be taught to put it down. He took it the answer would be that they were aiming at both these things, though one might be of greater importance than the other.

Then there was another point which really did constitute a difficulty. When a boy did a rider in an examination, he was so often faced with the fear that he would get no marks or full marks, according as to whether he did or did not succeed in finding a solution. It was difficult to aim at intermediate marks, as was the case in other subjects, and a boy might well waste twenty minutes in trying and failing to get a solution and get no credit at all for his attempt. Was there any way out of this difficulty?

Then in elementary rider-work he personally always had great difficulty in deciding exactly how to deal with the pupils doing rider-work at the very beginning. It tended so often to be mere axiomatic work. With that in mind he stressed the importance of what had been said by Mr. Lockwood as to calculation. He entirely agreed, and at the same time would like to criticise the paucity in the usual textbooks of calculations of real interest and value.

It had been suggested that merely one-third of the time devoted to geometry should be devoted to riders, the remaining two-thirds to be devoted to propositions, or he knew not what. He certainly thought far more than one-third should be devoted to riders, not necessarily theoretical.

Lastly, the speaker recorded his intense agreement with the point as to the pleasure of discovery. He supposed the greatest factor in all teaching was that of teaching elementary research, whatever the subject taught. If one could send a boy to a library to read a geometrical textbook, or whatever it might be, and the boy discovered something for himself, then the teacher was satisfying the instinct for creative work, the greatest instinct human beings possessed, whether boys or men.

Mr. K. S. Snell (Harrow) thought that at the beginning stages a boy, in interpreting a rider, had to go through two processes: first, he had to realise that the result he was asked to prove was correct; secondly, he had to prove it deductively. The speaker put that order purposely because he did not believe the boy would ever prove a rider unless he was first convinced either by accurate drawing or, more frequently, by a reasonable sketch-drawing that the result was correct. So often one saw pupils proving a thing which was obviously untrue, indicating that they had not grasped the real meaning of the question and not realised that the proving was only a second stage, proving deductively a result which looked likely to be true. The first stage could be arrived at, as had been suggested, by the "discovery" method: asking the boy to discover for himself. Where that was possible it was to the good. But in the earlier stages it was asking too much for a boy both to discover and to prove. Therefore the first riders following calculations, which he strongly recommended, must be very simple and must be treated along the lines which Mr. Boon had suggested, of putting down clearly exactly what was given, referring to the theorem which the boy was supposed to use, and then saying: "Here are the rules of the game. You are given certain things. There is a certain theorem or group of theorems on which you are working, and your business, now that you realise that the result is correct, is to prove it; that is, to deduce it from what you have put down and from these theorems. You must tackle it as a game, following certain definite rules and so getting to the result." That was the only way in which he thought what he called "obvious riders" could be made interesting. Although for long he fought against obvious riders and thought one should use only the riders which were not obvious, he had come to realise that in point of fact,

with beginners, one could not do that. The riders must be obviously true and the beginners had to learn the rules of the game : to put down what they were given and deduce the facts from theorems. In that connection Mr. Snell added that it was important to select one-step riders, and important to put the riders as riders on definite theorems.

As regards practical methods, one of the difficulties which boys experienced was their dislike of writing so much. If all the riders the boy did had to be written out fully, then he would naturally get rather tired of the process. Thus, although the writing was the essential thing to aim at eventually, the pupil must naturally have considerable practice in working out riders quickly by drawing figures, marking what was given instead of writing it down, and then proving the result, writing only an outline of the proof; for example, stating which triangles were congruent. That could be done as class work by tackling a rider and asking the boys to draw figures. After the lapse of two minutes one could draw the figure on the board and mark what was given and find out by a show of hands how much the boys agreed, and whether their figures showed the required result to be reasonable ; leave them to go on with the next stage and, again after two minutes, proceed to do it on the board. That was a little slow for faster boys and it would be varied by giving three or four riders to them. Give them ten minutes and then go through the riders on the board, with figures. One would thus limit the number written out.

Another point in connection with writing. Undoubtedly a large number of boys liked doing rider-work by means of constructions. They liked to express their geometrical reasoning, and to use their theorems in this way by drawing accurately. He found that owing to lack of time he was apt to omit that side of riders. Nevertheless, it was very important.

Mr. M. P. Meshenberg (Tiffins) appealed for a wider use of the senses in rider-work, as, of course, in all branches of education, where, however, one chiefly appeals to the eye and the reason. There had been very little use made, in geometry, of that valuable source of mental stimulus more or less resident in the ear. He had found it extremely useful to develop a sort of staccato version of the theorems, in a form in which the ear was stimulated. He taught boys : " If $PA = PB$, then P is—somewhere." The boys always knew where. He found that the rider which would otherwise prove difficult seemed to be easy if in reading the question the boy came across a familiar tune ; he pricked his ears up and said he had heard that somewhere. That system could be extended a great deal.

It was not at all a bad thing to teach boys that a semicircle was given them for one purpose only, and that was to give them right angles. There was practically no other use of a semicircle for boys when doing elementary rider-work. When a boy saw a figure and he asked him what that figure was particularly rich in, and the boy replied " A lot of right angles ", he said " Well ? " and the boy said

" A lot of semicircles ". He said again " Well ? " and the boy said " Diameters " and he made the boy look for diameters. It did not matter if there were no right angles in the question—the boy would then realise that his first thought was not good and would think again.

Another good example was mid-points, and his experience in that connection was somewhat amusing. If years after the boys had left his school he saw them and said " Mid-points ", they almost invariably barked back " Parallels " ! Parallels ? Well, they meant a lot of things but never meant mid-points to the boys unless there was one mid-point already in the figure. Then they looked for a second. He had frequently succeeded in getting B Forms to give him half-a-dozen boys who could prove the medians of a triangle concurrent as a rider by drawing the figure. There were plenty of youngsters who realised that they were mid-points, but no parallels. They could quickly be led to require a parallelogram, one of whose objects in life is to give bisected lines. He always tried in his figures to distinguish between what was given and what was put in by the different qualities of his lines, *e.g.*, dotted, fine, heavy or coloured.

He rather liked what Mr. Tongue had said about grouping theorems in sets according to what they did. He made boys say what were the theorems that gave them mid-points, and again he used staccato and shorthand names for theorems : mid-points given by (1) diagonals of parallelogram, (2) intercept theorem, (3) centre of circle and chord, (4) isosceles triangle.

He had found the appeal to the ear very useful. In fact he went so far as to say that he made an analysis of many years' papers in the London General and that some 70 per cent. of the riders set for the London General could be got out in outline merely by reading the question, before the boy put his pen to paper to draw a figure. The teacher read the question and said : " You are given a circle ", and *sotto voce* the boy remarked " Equal angles ". The circle was given him to get equal angles ; and he read on further : " and a point on a diameter ". " Right angles " said the boy. " You join certain lines and are then asked to prove that certain angles are equal." " Angles equal ? " says the boy. " Why, it's as easy as shelling peas. I have right angles and a circle. It's easy." And so it was.

There were one or two little points he always made with boys beginning their rider-work, to try to build them up into habits. In the first beginnings of rider-work he said, " Never try to draw an accurate figure to measure ". He thought that was a vicious method to begin on, but there he was prepared to find himself in the minority. After all, measurement proves that the angles of an isosceles triangle are *never* equal. The boys had to draw a reasonably good figure, and were to be guided by his second early slogan, " Never put into your figure what is not expressly given in the question ". He wondered how many were bothered by little boys who drew figures for a rider about a triangle and found they had drawn an equilateral triangle. Or if the boys were told to draw a quadrilateral they drew a trapez-

ium or a rectangle ; for parallelograms they invariably drew rhombuses—he did not know whether that was the right plural! He always made sure that boys never put into a figure what was not given in the question.

Then the four B's. Beginners invariably either drew large figures with tiny letters so that one's eyes got tired in travelling thousands of miles from letter to letter, or else they drew little figures and equally tiny letters and the whole thing was a nightmare. He made the boys use big bold black block letters. Incidentally, several of the gentlemen who had this morning given illustrations on the blackboard had offended against that rule! The small figure became quite clear if block letters were put in, whereas a large figure would look atrocious if lettered with small letters.

He appealed for the type of rider on which a paper had been published a couple of years ago in the *Mathematical Gazette*, to prove things not to be true or to be untrue : the latter was better. " Given a certain figure and certain data deduce that those two lines or angles cannot be equal or that this angle cannot be a right angle."

That was most instructive. The boys liked it. It gave them a feeling of really setting out to find something out. He felt the stressing of the research quality of early riders was largely illusory. He wondered how much research quality resided in the teacher's rather than in the boy's mind.

Mr. P. Fletcher (Cheltenham) asked : " Parallels, circles, what do they give you ? Angles. What are you going to do with the angles ? Draw a large clear figure and mark them." If a question spoke of an isosceles triangle, nearly everyone would mark the equal base angles in one way or another ; it was often convenient to call one of them α and the other α_1. One could then write down " $AB=AC$ (data) ; therefore $\alpha=\alpha_1$ ". But if one were asked to *prove* that two angles are equal, it would be best to call one of them α and the other β and to write " to prove $\alpha=\beta$ ".

Example. In the triangle ABC, $AB=AC$; AB is produced to D so that $BD=BC$; prove that $\angle ACD=3\angle ADC$.

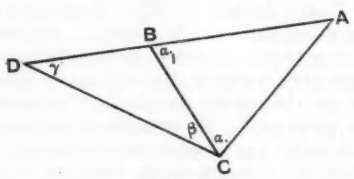


FIG. 8.

To prove : $\alpha+\beta=3\gamma$.

Proof. $BC=BD$ (given).

Therefore $\beta=\gamma$.

Therefore we have to prove $\alpha=2\gamma$.

But $\alpha = \alpha_1$, since $AC = AB$ (given)
and $\alpha_1 = \beta + \gamma$ (exterior angle of triangle BCD)
$= 2\gamma$.
Therefore $\alpha = 2\gamma$.
Therefore $\alpha + \beta = 3\gamma$. Q.E.D.

Mr. Snell had said that boys did not like writing. He would go one step further and say that schoolmasters did not like reading. Why should it always be necessary to give an angle a long name, $\angle PQR$, instead of just calling it α? If boys did not follow the admirable rule of the four *B*'s, one often had to spend time deciding whether a boy was thinking of $\angle ABC$ or of $\angle APO$; and even if his writing was clear, it was nevertheless a tedious business to follow a whole page of work in which every angle mentioned needed four signs to describe it instead of only one. The simplest case of a well-known theorem would then be written out like this:

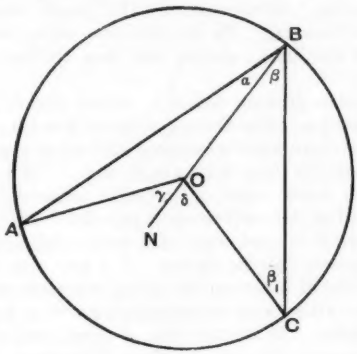


FIG. 9.

To prove: $\angle AOC = 2\angle ABC$,
that is, to prove $\gamma + \delta = 2(\alpha + \beta)$.
Proof. $OB = OA$ (radii of circle).
Therefore $\alpha = \alpha_1$.
Also $\gamma = \alpha + \alpha_1$ (exterior angle of triangle ABO)
$= 2\alpha$.
Similarly $\delta = 2\beta$.
Therefore $\gamma + \delta = 2(\alpha + \beta)$. Q.E.D.

He had proved quite definitely, as far as his own experience went, that the power of boys, even of stupid ones, to solve riders had been very greatly increased by the adoption of a technique of the type he had hinted at; and the amount of reading which he had had to do had been very much reduced.

Mr. J. Katz (Croydon) thought that what was damping down the experimental and constructive work (the rider-work leading up to a proposition) was the fact that so many textbooks retained the old-fashioned Euclidean headings for propositions. Those headings were inspired by the statical spirit of Platonism and of Greek mathematics. Consider this heading: "The opposite sides of a parallelogram are equal"; that type of heading was still to be found in a large number of textbooks. The Greek spirit spoke very clearly in that type of heading; there was an ideal form in heaven, a prototype, and one's business was to give a facsimile of it. He suggested that if really good rider-work was to be done, it would be necessary to alter the form in which the headings of the propositions were stated. Take the proposition on the parallelogram to which he had just referred. He got the class to put it in the following form: "If a quadrilateral is constructed with both pairs of opposite sides parallel, then those opposite sides will of themselves come equal". And what was also important, the heading was not formally enunciated until the boy had constructed the parallelogram in the way laid down in the heading. He got the boy, using his ruler and set-square, to draw the lines parallel, and then the boy saw that they came equal.

There was a still greater difficulty when one did the converse. How was one going to state the converse of this proposition so as to make it perfectly clear what was given and what was to be proved? He suggested that the right way was to say: "If a quadrilateral is constructed with both pairs of opposite sides equal, then these opposite sides will of themselves come parallel". He did not get the class to formulate it in that way until with ruler and compass they had actually constructed the figure. If a boy was writing out the proposition, he would insist on his using compass and ruler to construct the figure. Otherwise the whole proposition was a fraud. Before the proposition was written out, a great deal of experimental work with Meccano was done. The boy took four pieces of Meccano and made the opposite sides equal; he at once realised that those sides having been made equal, the figure of itself came to be a parallelogram.

Ought there not to be introduced into the headings of propositions the dynamic spirit of modern mathematics? Are we not retaining far too much of the static spirit of orthodox Greek geometry?

Mr. S. Inman (Isleworth County School) did not know why it was so customary to speak only of riders. No speaker so far had mentioned the same kind of constructive reasoning as could be obtained from what was generally called the constructional problem. That aspect seemed to be neglected. When looking at the report issued in 1924 by the Mathematical Association on the Teaching of Geometry he could find no reference to that point, and it seemed to him that the value that could be got out of the reasoning in working out a problem was more useful than that in working out riders. After all, what type of reasoning was used in work outside of geometry? He thought

it would be agreed that in nearly every case it was the analytical type of reasoning, and that was the type one used in a problem.

Supposing a scientist wished to discover certain properties. He knew what he wanted. For example, he might wish to find a substance which had the same ductility as metal and which would stand a lot of heat and so on and be a non-conductor. Or if a detective wished to discover the perpetrator of a crime, he knew it had been committed and he had to work back to the person who did the crime. The following are elementary examples of the type of problem the speaker had in mind :

(*a*) Given P, find QR so that $PQ = PR$ (Fig. 10 (i)).

(*b*) Given a line AB, find a point C on AB so that $AC^2 = 2CB^2$.

(*c*) Construct a triangle, given the perimeter and the base angles.

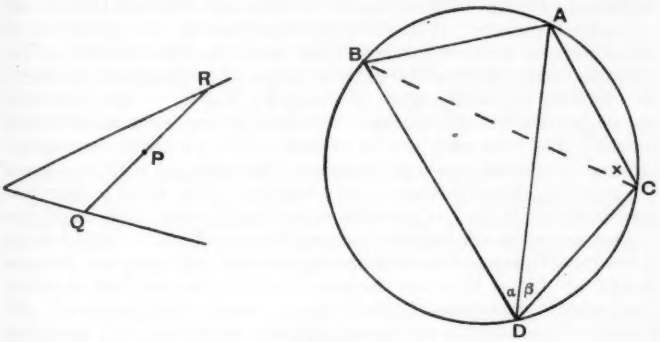


FIG. 10.

To solve them one had to apply the analytical type of reasoning. It was the same as reconstructing the crime and working backwards.

In solving riders, the pupil had to make a jump somewhere, and the point was that he would never succeed in making that jump unless he had what the speaker described as " a certain knowledge of the theorems involved ". For example, in Fig. 10 (ii) the proof that given $AB = AC$ then $\alpha = \beta$ depended on angles in the same segment being equal, but unless the boy knew the theorem thoroughly he would never succeed in making that jump. Although the boy thought he knew the theorem he found a great deal of difficulty in seeing that α and x were equal. It was necessary to see that the theorems were really known by the boy. There should be some elementary exercises in connection with each theorem, making that easy. For instance, when the theorem mentioned was taken the pupil should be asked to point out equal angles when a figure was turned round in different positions.

Coming back to the problem, one could prove that α and x were

equal and so on. There were certain things given and certain things to be proved. One way was to travel from the given in a straightforward direction until one got to the conclusion. A second way was to start from the end and work backwards, step by step, though not continuous steps, but in a series of jumps, to the given. A third way was to work from each end until a juncture was made.

Miss Feber (Manchester High School) said she had one quarrel to make with the speakers, namely that riders, at any rate in girls' schools, were given in the syllabus absolutely apart from theorems. She did not know whether it was the case with boys—it certainly was with girls beginning rider-work in geometry—that they always wondered why theorems should have such a particular distinct identity from riders. The speaker did not think that that fact was explained by very many teachers. She had always found that the best thing to do was to teach the theorems at first as riders. As was mentioned by one of the speakers, it was very difficult to find easy riders for beginners. If one took the rider-work as an introduction to theorems and worked on those lines there was less difficulty. For example, after proving the exterior angle of a triangle equal to the two interior opposite angles, it is easy to lead up to the proof that the angles of a triangle are equal to two right angles, taken at first as a rider. The next step was to explain to the pupil why one distinguished particular riders as theorems. She thought that step one of the most important in theoretical geometry. One made it clear that certain theorems had to be taken as necessary steps. One could give a rider, say on parallelograms, and ask the pupils what would happen if one tried to prove that without any theorems to refer to. But one should not forget that the theorems must be proved first as riders. That would, the speaker thought, help towards doing away with the fashion of learning the theorems by heart, which was very prevalent in girls' schools ; probably more so than in boys' schools.

Mr. E. H. Lockwood said that he was very grateful for the many suggestions that had been made for making rider-work easy. That was the first essential, particularly with beginners. One had to look for a large number of simple riders and try to make them as easy and attractive as possible. It had, for instance, been mentioned that it was useful to teach absolute beginners the formal routine for putting down the data and whatever was to be proved. The suggestion about the notation for angles was also helpful, and most important too was the proposal that pupils should, as a rule, be convinced of a fact before they prove it.

It had been asked, during the discussion, what was the aim of rider-work : whether it was the getting of the solution or the putting down of the steps. He would say rather that it was to make the pupils think for themselves, to get them started on original geometrical thought. It was asked also what was to be done with the other two-thirds of the time if one-third was spent on riders. The suggestion he had heard was that, very roughly, one-third should be spent on riders, one-third on theorems and one-third on some type of con-

structional work. But he thought several speakers had emphasised that both theorem learning and constructional work should be closely akin to rider-work. The methods right through geometry should be those they had been discussing for rider-work, so that really they were giving rider-work much more than one-third of the time ; they were giving it practically all the time.

He felt he must disagree almost completely with nearly everything Mr. Meshenberg had said. For instance, he said he never allowed his boys to draw accurate figures. Consequently it was not surprising that he should say also that he thought the research aspect of school geometry was an illusion. It occurred to the speaker that the " discovery " aspect of the work might very easily be swamped by the teaching aspect (especially if the teacher had a magnetic personality). And once they had embarked on teaching rather than allowing pupils to do things for themselves it was only too easy to slip into the method of teaching by ear, which was almost indistinguishable from what used to be called learning by rote !

The President felt sure all would agree that the discussion had been most interesting and, on behalf of the audience, he thanked Mr. Lockwood and Mr. Tongue for the papers they had read, and the various speakers who had contributed to the discussion.

1047. The Three-Letter Trick !—*Daily Mail*, 1935.

Sir,—In view of the rapidity with which the three-letter car registration marks are being used, because of the restriction of the numerals to three figures, perhaps a reader would inform me of the number of three-letter combinations it is possible to have with our alphabet of twenty-six letters.—September 9, 1935.

Sir,—There are 17,576 ($26 \times 26 \times 26$) different three-letter combinations possible with our alphabet of twenty-six letters. This gives us a total of 17,558,424 possible three-letter car registration marks.—September 13, 1935.

Sir,—Apparently your correspondent has overlooked the fact that in ascertaining the total of three-letter registrations, each letter may be repeated and such arrangements as AAA are permissible. The number, therefore, is not 15,600 (viz. $26 \times 25 \times 24$) as he stated, but $26 \times 26 \times 26$, that is, 17,576.—September 16, 1935.

Sir,—The correct answer is 70,200.

Sir,—The combinations possible are $26 \times 25 \times 24$ giving 15,600, not $26 \times 26 \times 26$. These 15,600 can have 999 numbers each, giving 15,584,400 possible three-letter registration plates.—September 16, 1935.

Sir,—The number of three-letter combinations that can be made out of a twenty-six-letter alphabet is 15,600 only ($26 \times 25 \times 24$), which, with the three-figure restriction, gives a total number of 15,584,400 combinations available for car registration purposes.—September 17, 1935.—*Daily Mail*, September 1935. [Per Prof. L. M. Milne-Thomson.]

1048. Conscious not only of material objects, but of what is passing in his fellow-Martian's mind—and this for an area of many hundreds of cubic yards. —G. Du Maurier, *The Martian*. [Per Miss H. P. Hudson.]

EXHIBITION OF MATHEMATICAL FILMS.

AT THE ANNUAL MEETING OF THE MATHEMATICAL ASSOCIATION, JANUARY 3RD, 1936.

The President thought it would be agreed that when teachers were asked whether it would be helpful to them in their teaching of mathematics to have films shown in school, it would be of advantage if they were able to say that they had seen mathematical films. Miss Punnett had selected a few films of the type likely to be of use and they would be shown with a view to eliciting an expression of opinion as to what was really needed.

Miss Punnett explained that the suggestion of showing films suitable for schools had been made by Mr. Holmes, a member of the Association and a Board of Education Inspector, after the last Annual Meeting. As a result, she had obtained the approval of the Programme Committee and seen various films with a view to choosing some that might be shown on that occasion as likely to be useful in the teaching of mathematics. There had been no opportunity for exercise of any large selection on her part. In the first place, there were very few mathematical films to select from and she had been unable to obtain access to all of them. Secondly, it had been difficult to decide what to select. At a private show at Kodak, Ltd. she had seen two films which seemed likely to be of use and these would be shown. One professed to depict the principles on which Einstein's theory of relativity was based. She had watched for any sign of its connexion with Einstein or his theory of relativity and had come to the conclusion that either her ideas of that theory or those of the film maker were somewhat wide of the mark. It seemed, however, that the first part of that film might be useful for a purpose different from that intended, namely to enable young children to understand what, in ordinary parlance, was called relative motion—something it was not at all easy to make clear to them. Miss Punnett added that since the film consisted of two parts and took nearly half an hour to show, she proposed to leave the second part to be taken at the end if time permitted.

The second film seen at the Kodak view was illustrative of frequency curves and appeared likely to be helpful for pupils beginning to understand how frequency curves might be built up.

She had consulted various people in selecting the films, including Mr. Lauwerys, who would be projecting the films chosen. She hoped he would take part in the discussion, because he was an authority on the subject in general, having edited a book published by Christopher's entitled *The Film and the School*. He and others had strongly advised her to include some Dance-Kaufmann films (now taken over by Steuart films), since they seemed to be those most likely to be of real use to teachers of mathematics.

Miss Punnett mentioned one film she had rejected, a Kodak film on mensuration. She had not actually seen the film, but had gathered

from a full description of it that it showed how a cube could be built up by small cubes—or, rather, a rectangular prism by small rectangular prisms, how an area could be built up by smaller areas. All this seemed undesirable, because, as far as the elementary part went, it seemed quite unsound educationally to allow children to sit and enjoy themselves lazily watching a film of something they might do with their fingers. But when it came to a line being made up of a series of curves and an area made up of a series of lines she had gasped a little, because, surely, that did not belong to that stage. There seemed to be a mixing of two things of different stages, neither of which was suitable for film treatment.

Miss Punnett concluded by expressing the hope that there would be some discussion on the films shown, particularly an expression of opinion as to the kind of illustrations teachers of mathematics needed in films. It seemed to her that in mathematics, as well as in other subjects, film-producers had been making films they thought were wanted without knowing what the teachers thought. In emphasising the fact that the producers need guidance from those who use the films, Miss Punnett added that she knew the Film Institute and others would wish for an expression of opinion as to what users really did want and would pass it on to the producers. If members who had ideas on the subject would send them to her she would forward their communications to the proper quarter. It was, in short, obviously necessary that there should be an effort to secure production of the right type of mathematical film and not merely tame use or rejection of what the film companies decided to produce.

It was clear that the Mathematical Association ought to play an important part in influencing the form mathematical films should take.

The Dance-Kaufmann films were not expensive to purchase, mostly varying from 10s. to £1, the longest films costing more. The address at which the Dance-Kaufmann films could be purchased was: Steuart Films, The Studios, North Fambridge, Chelmsford, Essex. Those films could not be hired.* The Kodak films could be hired from Kodak, Ltd., Kingsway, W.C. 2., at the rate of 3s. 6d. per reel per day. The Einstein film, consisting of two reels, would cost 7s. per day.

The following films were then shown: two Dance-Kaufmann films entitled "Mathematical Processes" and "Harmonic Motion" and a film made under the direction of Mr. Fairgrieve, until recently a member of the staff of the Institute of Education and a geographer interested in mathematics. This film depicted various cases in which two points moved in ways definitely related to one another; it was intended to introduce the idea of functionality and to lead up to its expression in a graph.

These were followed by two films produced by Kodak, Ltd. The first was said to illustrate Einstein's principle of relativity. It

* That decision has since been rescinded; the Dance-Kaufmann films can now be hired at 2s. 6d. per reel per day.

began with examples of man power superseded by machine power, gave illustrations of appearances deceptive to the senses, and, finally, invited the audience to view the Earth from somewhere in starry space, while relativity of size was illustrated by the Moon, Earth and Sun in comparison with the planet Betelgeuse.

The second Kodak film illustrated frequency curves by grouping small objects according to size into two, then into four, areas; subdivision into still smaller groups showing that as the number of areas increased there was a tendency towards a curve.

A film both devised and executed by Mr. B. G. D. Salt was also shown. It was an ingenious moving diagram of the euclidean proof of the theorem of Pythagoras; it showed successively, by picking them out separately, the equality of the various angles and figures required in the course of the proof.

Discussion.

Mr. G. L. Parsons (Merchant Taylors') thought the films suffered from two contradictory faults. It seemed that the ellipse film would have been improved if there had been three stages instead of the continual monotony of the movement. There should be some easily assimilated picture of the apparatus and, possibly, somebody trying to trace the mid-point by sliding a ruler; then a picture of the apparatus by which the sliding was contrived. The film would then be broken up into two or three processes, so that the attention of the child would not be distracted by continuous movement. The Einstein film showed that fault corrected to such a degree that it became absurd. In order that some point should be made about relative motion one saw, first, men shovelling dirt and then machines shovelling dirt. Then trains and aeroplanes and skyscrapers. Mickey Mouse had not been brought in but he, personally, had expected to see him. Between those two extremes there was a mean which should be aimed at, especially in mathematical films, namely relation, in some way or other, with the mathematical concept illustrated.

Mr Hope-Jones (Eton) thought it would have been better if, instead of showing the ellipse ready made, it had been possible to see the ellipse being traced by the moving point so that it would arrive as the point moved. After going round about twice the line might have stood still to produce itself for about one-third of its own length and then gone round again, and the new end might have described a new ellipse, illustrating that the thing worked not only when the point was between the two original ends, but also when it was on the produced part of the line that was sliding round.

He liked the frequency-distribution, but the equation in the north-east corner was not legible. It might have been put in the middle first, and after all had read it it could have been put in a minor position in the north-east corner.

Pythagoras was great fun, but why call a right-angle 90° ? The

right-angle got there before the degree, and he did not think one wanted to think of a right-angle as defined as being made of 90°.

Mr. S. F. Trustram (Smethwick) disagreed with Mr. Hope-Jones' remarks as to the frequency curves. He was of opinion that a subject such as that could be best dealt with by ordinary demonstration. Those able to understand frequency curves would be quite well able to understand demonstrations given by an ordinary schoolmaster. The assistance given by the film was only very slight, though it could be used for recapitulation.

Mr. S. Inman (Isleworth County School) said when he told some of his colleagues who worked with him that there were chalk compasses for use they replied that they never used a compass. He thought the same might apply to some of the films. Unless it was worth while getting the apparatus out, it would not be used. A film would have to illustrate something which it was difficult to illustrate with chalk and blackboard.

Mr. Lauwerys pointed out, in answer to Mr. Parsons and Mr. Hope-Jones, that it was difficult to judge the value of films without remembering the purpose for which they were actually produced. The film showing the production of ellipses, for instance, was meant to be taken purely as part of a lesson. It was not meant to replace the lesson, but simply to be in the nature of a blackboard illustration. It was left to the teacher to do whatever else he could to produce the apparatus or attempt to draw the ellipse in that sort of way. It was a moving blackboard diagram.

The Einstein film was meant to be the sort of thing that could be shown on Saturday night to the whole school and not merely to be used in the classroom. All the Dance-Kaufmann films were meant to be a representation of cyclic processes : things that happened over and over again, things that could be put on an endless band. It had been found that in order to get cyclic films on to a projector such as he had used it was necessary to provide apparatus which would render the film cheap ; one could either use a peg at the top or else purchase the standard addition to the projector which cost about £2 10s. It appeared to be too much trouble for people to use the cheap apparatus and no one bought the standard addition, so that the films did not sell. The makers therefore put them on to 100 ft. reels, such as had been shown, which, incidentally, added somewhat to the cost.

Time being available, the second reel of the Einstein relativity film by Kodak, Ltd. was shown. It depicted comparisons of the paths of a bullet, first relative to the Earth and second as seen by an observer in space ; the speed of other planets in relation to the speed of the Earth and the Sun, and the unvarying speed of light. It suggested that the theory opened up " an unlimited field for speculations, dreams and phantasies ", and the film concluded with the caption that " a man shot from the Earth finds his former past is now his future ".

The exhibition concluded with a most hearty vote of thanks to

Miss Punnett and Mr. Lauwerys for the trouble they had taken in selecting and showing the films.

FILM SUB-COMMITTEE.

Largely because of the interest shown in the demonstration of mathematical films at the last Annual Meeting, the General Teaching Committee has appointed a film sub-committee. Its duties are to watch the production of mathematical films and to collect such information as may make it possible to stimulate and guide that production so that it may be of most use to teachers.

The evidently wide interest in the subject encourages the hope that many members of the Association may have opinions to express and suggestions to offer that would be of value to the sub-committee; suggestions for subjects for specific films would be very welcome.

We shall be greatly obliged if members will send as many suggestions as they can, if possible before the end of June, to

MISS PUNNETT,

17 Gower Street,

London, W.C. 1.

1049. A prime number, you remember (I didn't), is one which is divisible only by itself and 1—*e.g.* 3, 7, 17, 23.

Chicago's Samuel I. Krieger challenges the world to find a longer prime number than his 72-digit 231,584,178,474,632,390,847,141,970,017,375,815,706,539,969,331,281,128,078,915,826,259,279,817. Why not have a shot at it?—*Daily Express*, December 27, 1934.

Two queries. (*a*) Is this definitely known to be prime? (*b*) If not, is not

$$2^{127}-1$$

the largest known prime? [Per Mr. R. H. Walton.]

1050. The limit is attained!

Kenneth is making the acquaintance of decimals and has been asked to divide ten by three. He proceeds:

"Three into ten, three—decimal point—"

"Right. Go on."

"Carry one—three into ten, three—another three, and another. Why, they are all threes."

"Yes, indeed."

"Well, how long do they go on?"

"As long as you like."

"What, for a million years?"

"Yes, if you like."

"No, not for a million years."

"Why not?"

"Well, you see, by that time they'll know much more about mathematics than we do and they'll soon put a stop to that."

A PROPERTY OF THE SECANT.

By N. M. Gibbins.

1. When we proceed to obtain the equation of the join of points on the ellipse $x^2/a^2+y^2/b^2=1$ whose eccentric angles are θ and ϕ, we find that $\cos\frac{1}{2}(\theta-\phi)$ is a factor of each term; and similarly a common factor is found to exist for any other case of parameters. We may therefore suspect the existence of a common factor when we find the equation of the secant in the general case. It is proposed to find this common factor and to apply the simplified equation to the solution of a well-known problem.

2. The equation of the join of the points (x_1, y_1, z_1) and (x_2, y_2, z_2) is

$$x(y_1z_2-y_2z_1)+y(z_1x_2-z_2x_1)+z(x_1y_2-x_2y_1)=0, \quad \ldots\ldots\ldots\ldots\text{(i)}$$

and if the points lie on the conic $S=0$, we have

$$\begin{aligned}\Delta(y_1z_2-y_2z_1)^2&=\begin{vmatrix} a & h & g \\ h & b & f \\ g & f & c \end{vmatrix}\begin{vmatrix} 1 & 0 & 0 \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}^2\\ &=\begin{vmatrix} a & h & g \\ X_1 & Y_1 & Z_1 \\ X_2 & Y_2 & Z_2 \end{vmatrix}\begin{vmatrix} 1 & 0 & 0 \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}\\ &=\begin{vmatrix} a & X_1 & X_2 \\ X_1 & 0 & P_{12} \\ X_2 & P_{12} & 0 \end{vmatrix}\\ &=P_{12}(2X_1X_2-aP_{12}),\end{aligned}$$

where $X_1=ax_1+hy_1+gz_1$, etc., and $P_{12}=ax_1x_2+\ldots$;

with two similar expressions for $\Delta(z_1x_2-z_2x_1)^2$ and $\Delta(x_1y_2-x_2y_1)^2$.

The common factor is thus $\sqrt{(P_{12}/\Delta)}$, and equation (i) reduces to

$$x(2X_1X_2-aP_{12})^{\frac{1}{2}}+y(2Y_1Y_2-bP_{12})^{\frac{1}{2}}+z(2Z_1Z_2-cP_{12})^{\frac{1}{2}}=0, \quad \ldots\text{(ii)}$$

where the positive values of the square roots are to be taken. We also have that

$$\begin{aligned}&\Delta(z_1x_2-z_2x_1)(x_1y_2-x_2y_1)\\ &\quad=\begin{vmatrix} a & h & g \\ h & b & f \\ g & f & c \end{vmatrix}\begin{vmatrix} 0 & 1 & 0 \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}\begin{vmatrix} 0 & 0 & 1 \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}\\ &\quad=\begin{vmatrix} h & b & f \\ X_1 & Y_1 & Z_1 \\ X_2 & Y_2 & Z_2 \end{vmatrix}\begin{vmatrix} 0 & 0 & 1 \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}=\begin{vmatrix} f & Z_1 & Z_2 \\ Y_1 & 0 & P_{12} \\ Y_2 & P_{12} & 0 \end{vmatrix}\\ &\quad=P_{12}(Y_1Z_2+Y_2Z_1-fP_{12}).\end{aligned}$$

We have therefore in addition three identities

$$(2Y_1Y_2-bP_{12})^{\frac{1}{2}}(2Z_1Z_2-cP_{12})^{\frac{1}{2}}=Y_1Z_2+Y_2Z_1-fP_{12},$$

the other two being written down in cyclic order.

3. We may apply this simplification to the solution of the following problem: a triangle is inscribed in the conic $S=0$, and two sides touch the conic $S'=0$; find the equation of the third side.

The condition that equation (ii) should touch $S'=0$ is

$$A'(2X_1X_2-aP_{12})+\ldots+2F'(Y_1Z_2+Y_2Z_1-fP_{12})+\ldots=0,$$

with the usual notation. The condition that the secant 1, 3 may touch $S'=0$ is this equation with the suffix 2 replaced by 3. These equations are linear in x_2, y_2, z_2 and x_3, y_3, z_3 respectively. Hence the equation of the secant 2, 3 is

$$A'\{2X_1X-a(X_1x+Y_1y+Z_1z)\}+\ldots+\ldots$$
$$+2F'\{Y_1Z+YZ_1-f(X_1x+Y_1y+Z_1z)\}+\ldots+\ldots=0,$$

or, rearranging,

$$X_1\{2(A'X+H'Y+G'Z)-\Theta'x\}+Y_1\{2(H'X+B'Y+F'Z)-\Theta'y\}$$
$$+Z_1\{2(G'X+F'Y+C'Z)-\Theta'z\}=0, \quad \ldots\ldots\ldots\ldots\ldots\ldots\text{(iii)}$$

where $$\Theta'=aA'+bB'+cC'+2fF'+2gG'+2hH'.$$

The direct calculation of the envelope of this line will be found at the end of this article.

4. A slight modification of the preceding method may be employed to give a neat solution when two of the sides of the triangle inscribed in $S=0$ pass respectively through given points.

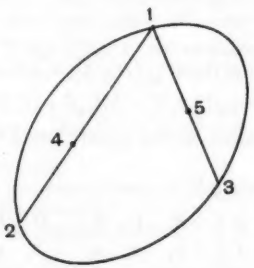


FIG. 1.

In the figure the numbers refer to suffixes of x, y, z. We have $|x_4y_1z_2|=0$ and $|x_5y_1z_2|\neq 0$. Hence the condition that 4 may be on the secant 1, 2 is

$$0=\begin{vmatrix} a & h & g \\ h & b & f \\ g & f & c \end{vmatrix}\begin{vmatrix} x_4 & y_4 & z_4 \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}\begin{vmatrix} x_5 & y_5 & z_5 \\ x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \end{vmatrix}$$

$$\equiv \begin{vmatrix} P_{45} & P_{41} & P_{42} \\ P_{15} & 0 & P_{12} \\ P_{25} & P_{21} & 0 \end{vmatrix}$$

$$\equiv P_{45}(-P_{12})^2 + P_{41} \cdot P_{25} \cdot P_{12} + P_{42} \cdot P_{15} \cdot P_{21}$$

$$\equiv P_{12}(P_{14} \cdot P_{25} + P_{15} \cdot P_{24} - P_{12} \cdot P_{45}).$$

Here P_{12}/Δ is a factor of the product of the determinants $|x_4y_1z_2|$ and $|x_5y_1z_2|$, and the required condition reduces to

$$P_{14} \cdot P_{25} + P_{15} \cdot P_{24} - P_{12} \cdot P_{45} = 0.$$

Interchanging 4 and 5, and changing 2 into 3, the condition that 5 may lie on the secant 1, 3 is

$$P_{15} \cdot P_{34} + P_{14} \cdot P_{35} - P_{13} \cdot P_{54} = 0.$$

Hence the equation of the secant 2, 3 is

$$P_{14} \cdot P_{05} + P_{15} \cdot P_{04} - P_{01} \cdot P_{45} = 0,$$

where x_0, y_0, z_0 are current coordinates. This may be written

$$X_1(x_4 \cdot P_{05} + x_5 \cdot P_{04} - x_0 P_{45}) + \ldots + \ldots = 0,$$

and its envelope subject to $\Delta S_1 \equiv AX_1^2 + \ldots + 2FY_1Z_1 + \ldots = 0$ is

$$a(x_4 \cdot P_{05} + x_5 \cdot P_{04} - x_0 \cdot P_{45})^2 + \ldots$$
$$+ 2f(y_4 \cdot P_{05} + y_5 \cdot P_{04} - y_0 \cdot P_{45})(z_4 \cdot P_{05} + z_5 \cdot P_{04} - z_0 \cdot P_{45}) + \ldots = 0,$$

that is,

$$P_{44} \cdot P_{05}^2 + P_{55} \cdot P_{04}^2 + P_{00} \cdot P_{45}^2 + 2P_{45} \cdot P_{05} \cdot P_{04}$$
$$- 2P_{40} \cdot P_{05} \cdot P_{45} - 2P_{50} \cdot P_{04} \cdot P_{45} = 0,$$

or $$P_{00} \cdot P_{44} \cdot P_{55} = P_{00} \cdot P_{44} \cdot P_{55} + 2P_{04} \cdot P_{05} \cdot P_{45}$$
$$- P_{00} \cdot P_{45}^2 - P_{44} \cdot P_{05}^2 - P_{55} \cdot P_{04}^2$$

$$\equiv \begin{vmatrix} P_{00} & P_{04} & P_{05} \\ P_{40} & P_{44} & P_{45} \\ P_{50} & P_{54} & P_{55} \end{vmatrix},$$

that is, $$S_0S_4S_5 = \Delta \begin{vmatrix} x_0 & y_0 & z_0 \\ x_4 & y_4 & z_4 \\ x_5 & y_5 & z_5 \end{vmatrix}^2,$$

a conic having double contact with $S_0 = 0$ at the points in which it is cut by the join of 4, 5.

5. The same method gives an amusing variation of treatment in obtaining the tangent, chord of contact and pair of tangents.

For four arbitrary points we have the identity

$$\begin{vmatrix} a & h & g \\ h & b & f \\ g & f & c \end{vmatrix} \begin{vmatrix} x_1 & y_1 & z_1 \\ x_2 & y_2 & z_2 \\ x_3 & y_3 & z_3 \end{vmatrix} \begin{vmatrix} x_4 & y_4 & z_4 \\ x_2 & y_2 & z_2 \\ x_3 & y_3 & z_3 \end{vmatrix} \equiv \begin{vmatrix} P_{14} & P_{12} & P_{13} \\ P_{24} & P_{22} & P_{23} \\ P_{34} & P_{32} & P_{33} \end{vmatrix} \quad \ldots(\text{iv})$$

If now 2 and 3 are on the conic and 1 is on 2, 3 (Fig. 2), we have

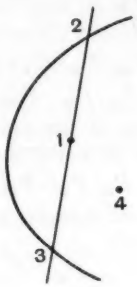


FIG. 2.

$P_{22}=P_{33}=0$ and $|x_1y_2z_3|=0$, while $\Delta\neq 0$ and $|x_4y_2z_3|\neq 0$. Hence from (iv)

$$0=\begin{vmatrix} P_{14} & P_{12} & P_{13} \\ P_{24} & 0 & P_{23} \\ P_{34} & P_{23} & 0 \end{vmatrix}$$

$$=P_{23}(P_{12}\cdot P_{34}+P_{13}\cdot P_{24}-P_{14}\cdot P_{23})$$

or
$$P_{12}\cdot P_{34}+P_{13}\cdot P_{24}-P_{14}\cdot P_{23}=0. \quad \text{.....(v)}$$

It is to be noted that in this case $|x_1y_2z_3||x_4y_2z_3|$ has the factor P_{23}/Δ.

If $3\to 2$, (v) becomes $P_{12}\cdot P_{24}+P_{12}\cdot P_{24}=0$, or $P_{12}=0$ since 4 is arbitrary. Thus $P_{12}=0$ with $P_{22}=0$ is the tangent at 2.

If now 4 is the point of intersection of the tangents at 2 and 3 (Fig 3), we have $P_{24}=0$, $P_{34}=0$, so that (v) now becomes $-P_{14}\cdot P_{23}{}^2=0$ or $P_{14}=0$, the chord of contact.

Lastly, if 3 and 2 are on the tangent at 1 and 4 is arbitrary, we have $P_{12}=0$, $P_{13}=0$, $|x_1y_2z_3|=0$; hence (iv) becomes

$$0=P_{14}(P_{22}\cdot P_{33}-P_{23}{}^2) \quad \text{or} \quad \text{"}SS'=P^2\text{"}.$$

6. The envelope of (iii) subject to $AX_1{}^2+\ldots+2FY_1Z_1+\ldots=0$ is

$$a\{2(A'X+H'Y+G'Z)-\Theta'x\}^2+\ldots$$
$$+2f\{2(H'X+B'Y+F'Z)-\Theta'y\}\{2(G'X+F'Y+C'Z)-\Theta'z\}+\ldots=0,$$

or
$$a(A'X+H'Y+G'Z)^2+\ldots$$
$$+2f(H'X+B'Y+F'Z)(G'X+F'Y+C'Z)+\ldots$$
$$-\Theta'\{X(A'X+H'Y+G'Z)+\ldots\}=-\tfrac{1}{4}S\Theta'^2.$$

In the left-hand side the coefficient of X^2 is

$$aA'^2+bH'^2+cG'^2+2fG'H'+2gG'A'+2hH'A'$$
$$-A'(aA'+bB'+cC'+2fF'+2gG'+2hH')$$
$$=-\Delta'(bc'+b'c-2ff').$$

The coefficient of $2YZ$ is

$$\begin{aligned} &aG'H' + bF'B' + cF'C' + f(B'C' + F'^2) + g(F'G' + C'H') \\ &\quad + h(H'F' + B'G') \\ &\quad - F'(aA' + bB' + cC' + 2fF' + 2gG' + 2hH') \\ &= -\Delta'(gh' + g'h - af' - a'f). \end{aligned}$$

Hence $$S\Theta'^2/4\Delta' = (bc' + b'c - 2ff')\,X^2 + \ldots + 2(gh' + g'h - af' - a'f)\,YZ + \ldots .$$

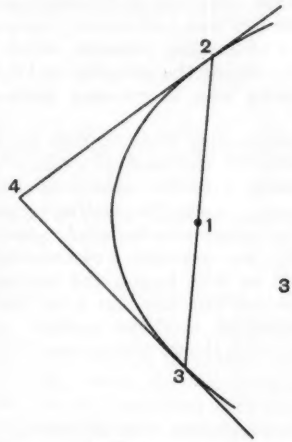


FIG. 3.

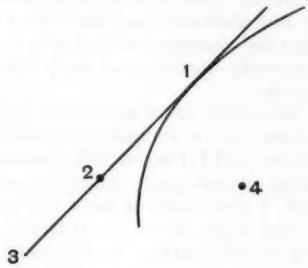


FIG. 4.

In the right-hand side of this the coefficient of a' is

$$cY^2 + bZ^2 - 2fYZ \equiv AS - \Delta x^2 ;$$

that of $2f'$ is $$-fX^2 - aYZ + hZX + gXY \equiv FS - \Delta yz.$$

Hence $$S\Theta'^2/4\Delta' = \Theta S - \Delta S'$$

or $$(\Theta'^2 - 4\Theta\Delta')\,S + 4\Delta\Delta' S' = 0,$$

where $$\Theta = a'A + b'B + c'C + 2f'F + 2g'G + 2h'H.$$ N. M. G.

1051. The enchantment of rhythm is obviously felt in music, the rhythm which is inherent in the notes and their grouping. It is the magic of mathematics, this rhythm which is in the heart of all creation, which moves in the atom and in its different measures fashions gold and lead, the rose and the thorn, the sun and the planets, the variety and vicissitudes of man's history. These are the dance-steps of numbers in the arena of time and space, which weave the *maya* of appearance, the incessant flow of changes that ever is and is not. What we know as intellectual truth, is that also not a perfect rhythm of the relationship of facts that produce a sense of convincingness to a person who somehow feels that he knows the truth ? We believe any fact to be true because of a harmony, a rhythm in reason, the process of which is analysable by the logic of mathematics.—Rabindranath Tagore, in "Sandhyā," the *Indian Journal of Statistics*, 1935, Vol. 2, p. 1. [Per Dr. J. Wishart.]

THE REHABILITATION OF DIFFERENTIALS.*

By G. Temple.

When the subject of the teaching of differentials was discussed by this Association at its Annual Meeting two years ago, I read a paper on the theoretical aspect of the subject. In that paper I maintained that it is futile to discuss the teaching of differentials until we have agreed upon the theory of differentials which we wish to teach, and I proceeded to summarise the three main theories of differentials. It was obvious that each of these theories was intrinsically more difficult and abstract than the theory of limiting processes which has become the classical method of introducing the calculus, and it was unanimously agreed that each theory was, in practice, quite unteachable.

At the same time, it became surprisingly clear at that meeting that there was a widespread demand for the reintroduction of differentials, and I, for one, left the meeting entirely convinced of their practical advantages. In fact, having come to the meeting prepared to scoff, I went home to hope and to re-examine the whole question.

At first sight, it appeared that it was impossible to construct a theory of differentials which should be both logical and teachable, but happily it proved possible to cut this Gordian knot and to formulate a theory of differentials which sacrifices neither logical rigour nor practical simplicity. It is this theory which I am venturing to expound this afternoon.

I. *Object of the Theory.*

In the first place, I should like to emphasise what is implied by a theory of differentials so far as my paper is concerned. I do *not* mean a theory which presupposes the definition of derivatives and which merely provides a justification for the notation of differentials. That is a comparatively trivial business; it has its undoubted uses and advantages, but it is not what I mean by a "theory of differentials". I mean a theory which is formally independent of the theory of limits and which yields a definition of a derivative as a ratio of differentials, so that the theory I am attempting to explain enables the calculus to be established quite independently of the theory of limits. The object of this paper is to show that such a theory can be made simple and self-consistent. I shall begin by summarising the leading points in this theory of differentials, then speak of the formal justification of the theory, and, finally, discuss its advantages and disadvantages.

II. *The Theory of Differentials.*

1. The basis of this theory is the introduction of infinitesimals by an explicit definition which gives their fundamental distinction from ordinary real numbers. If a and b are ordinary real numbers, no matter how small $|a|$ may be compared with $|b|$, there necessarily

* A paper and discussion at the Annual Meeting of the Mathematical Association, January 3rd, 1936.

exists a number N such that $|Na|>|b|$. This is the axiom of Archimedes. An (absolute) infinitesimal, η, is, however, defined to be a number such that

$$|N\eta|<1,$$

for all numbers N. Once it is admitted that such numbers exist the rest of the theory of differentials is merely a matter of elementary algebra and trigonometry.

Relative infinitesimals are defined in a similar manner—α is infinitesimal compared with A if

$$|N\alpha|<|A|,$$

for all N. The following examples provide simple illustrations of these concepts :

Ex. 1. If a is an ordinary positive number and $|N\eta|<a$ for all N, then η is an (absolute) infinitesimal.

For, if $|M\eta|>1$, for any integer M, then $|(aM)\eta|>a$.

Ex. 2. If η is an absolute infinitesimal, η is infinitesimal compared with η^2.

For, $$|N\eta|<1,$$

whence $$|\eta|.|N\eta|<|\eta|,$$

i.e. $$|N\eta^2|<|\eta|.$$

Ex. 3. If the arc of a circle is infinitesimal in length, then the chord of the arc is also infinitesimal, and the sag (sagitta) of the arc is infinitesimal compared with the arc.

For, since the chord is less than the arc, if θ is the circular measure of the arc

$$0<\sin\theta<\theta.$$

Hence, if $|N\theta|<1$, then $|N\sin\theta|<1$.

Also $$0<1-\cos\theta<2\sin^2\tfrac{1}{2}\theta.$$

Whence

$$|N(1-\cos\theta)|<2|\sin\tfrac{1}{2}\theta|.|N\sin\tfrac{1}{2}\theta|<2|\sin\tfrac{1}{2}\theta|<\theta.$$

2. The introduction of infinitesimals requires a modification or extension of the concept of equality. The problem is to describe the relation between two numbers, a and $a+\eta$, where a is finite and η infinitesimal. We can either invent a new name, such as "equivalence", to describe this relation or we can give a fresh definition of equality which shall include this relation. Originally I adopted the first alternative and defined two numbers, a and b, to be equivalent, if $(a-b)$ is an infinitesimal compared with a or b. But, on second thoughts, I think it is preferable to adopt the second alternative. After all, a and $a+\eta$ are the measures of two lengths which are experimentally indistinguishable, so why not call them equal! We therefore extend the concept of equality by defining that

$$a=b \text{ if } |N(a-b)|<|a| \text{ or } |b|, \text{ for all } N.$$

This extended definition of equality requires some caution in its use. Clearly, if $a=b$, it does not now follow that $a-b=0$, for $a-b$ may be an infinitesimal. Also if $a=b$ and $c=d$, it does not follow that $a-c=b-d$, as may be seen by considering the case in which $b=a+\eta$, $c=-a$ and $d=-a+\eta$. But, on the other hand, the following theorem is always true :

Ex. 4. If $a=b$ and $c=d$, then $ac=bd$.

For $$ac-bd=(a-b)c+b(c-d).$$

Now $$|N(a-b)|<|a| \text{ and } |N(c-d)|<|d|.$$

Hence

$$|N(ac-bd)|<|N(a-b)|c|+|Nb(c-d)|<|ac|+|bd|$$

and $$|N(ac-bd)|<|ac| \text{ or } |bd|.$$

Further illustration of the manipulation of differentials are provided by the following example :

Ex. 5. If η is infinitesimal, then

$$\sin\eta=\eta \quad \text{and} \quad \frac{1-\cos\eta}{\eta}=\tfrac{1}{2}\eta.$$

For, since the area bounded by a circular arc and two radii is less than the area bounded by the same radii and the tangent at the mid-point of the arc, if 2η is the angle of the arc, then

$$\tfrac{1}{2}\eta<\tfrac{1}{2}\tan\eta,$$

whence $$0<1-\frac{\sin\eta}{\eta}<1-\cos\eta=2\sin^2\tfrac{1}{2}\eta.$$

Therefore $$N(\eta-\sin\eta)<2N\eta\sin^2\tfrac{1}{2}\eta<\tfrac{1}{2}\eta^2<\eta.$$

Similarly,

$$N\left(\tfrac{1}{2}\eta-\frac{1-\cos\eta}{\eta}\right)=\frac{N(\frac{1}{2}\eta+\sin\frac{1}{2}\eta)(\frac{1}{2}\eta-\sin\frac{1}{2}\eta)}{\frac{1}{2}\eta}$$

$$<(\tfrac{1}{2}\eta+\sin\tfrac{1}{2}\eta)<\eta.$$

3. The application of these methods to the calculus depends upon the following definition : If η is an infinitesimal increment in the variable x, the corresponding differential of a function $f(x)$ is defined to be

$$df(x)=f(x+\eta)-f(x).$$

In particular $$dx=\eta.$$

Continuity and differentiability can now be defined concisely as follows :

(i) $f(x)$ is continuous if $f(x+dx)=f(x)$,

(ii) $f(x)$ is differentiable if there exists a function $m(x)$ such that $df(x)=m(x)\,dx$.

In these circumstances $m(x)$ is called the derivative of $f(x)$ and is a true ratio $df(x):dx$.

There are enormous practical advantages to be gained from a definition of a derivative as the ratio of two differentials. Those who attended the meeting of the Association two years ago must have been greatly impressed by the remarks of Mr. Tuckey, who said what we all thought but had not dared to say: that as long as we continue to teach our students to write a derivative as if it were a ratio of dy and dx, so long will they think that it really is a ratio. The great advantage of the definition given here is that it makes the derivative to be the ratio of two differentials, and thus removes the necessity of explaining that we are using for sentimental and mnemonic reasons a notation which is about three hundred years out of date.

From this definition we can proceed to differentiate all the elementary functions with very little trouble.

Ex. 6. If $f(x)=x^n$, where n is a positive integer,

$$\begin{aligned} df(x) &= nx^{n-1}\,dx + \tfrac{1}{2}n(n-1)x^{n-2}(dx)^2 + \dots \\ &= nx^{n-1}\,dx. \end{aligned}$$

If

$$\begin{aligned} f(x) &= \sin x, \\ df(x) &= \sin(x+dx) - \sin x \\ &= \tfrac{1}{2}\cos(x+\tfrac{1}{2}\,dx)\sin\tfrac{1}{2}\,dx \\ &= \cos x. \end{aligned}$$

The fundamental theorems of the derivative can be established in a few lines using the notation of differentials:

$$d\{cf(x)\} = c\,df(x);$$

$$d\{f(x)+g(x)\} = df(x)+dg(x);$$

$$\begin{aligned} d\{f(x)g(x)\} &= g(x)\,df(x) + f(x)\,dg(x) \\ &\quad + df(x)dg(x) = g(x)df(x) + f(x)dg(x); \end{aligned}$$

$$d\{1/f(x)\} = -\frac{df(x)}{f(x+dx)f(x)} = -\frac{df(x)}{\{f(x)\}^2};$$

$$dg(x) = \frac{dg}{df}\,.\,df(x).$$

Of course it is well known that the great advantage of this notation is the simplicity with which it expresses all the standard theorems relating to the properties of the differential coefficient. Here we have the additional advantage that the notation not only expresses the theorems simply, but actually suggests the method of proof.

So much for a brief sketch of the theory, for which I can justly claim that it is extremely simple and perfectly rigorous. I will now pass on to say something in justification of the theory.

III. *Justification of Theory.*

The method which I have adopted here for the introduction of infinitesimals is that which we all use when we lecture to elementary

students on real numbers and complex numbers. No one would dream of postponing the use of real numbers until a formal definition had been given. You would think it absurd if you were forbidden to use real numbers until you had mastered the theory of sequences or Dedekind sections, and equally absurd to postpone the use of complex numbers until the formal theory of ordered pairs had been studied. What we do is simply to postpone the existence-theorem for these kinds of numbers to a later stage. We summarise the properties of these numbers in a set of axioms and ask the students to make an act of faith in their self-consistency, the proof of which is postponed until later. This process has enormous advantages in that the student begins by acquiring a certain technical facility with the subject. But if you compel the unfortunate individual to begin by mastering a proof of self-consistency or an existence-theorem, it is extremely probable that he will make no progress whatever with either the logic or the technique of the subject. The separation of the study of structure and function is well recognised as a legitimate procedure in the case of real and complex numbers, and I suggest that we should follow the same procedure in dealing with differentials.

The formal definition of infinitesimals on which the whole theory depends does, of course, require to be justified at some later stage. It is necessary to show, sooner or later (preferably much later), that there are entities for which that definition is valid. When students have begun to study limiting processes, which can be postponed for at least a year, the formal justification can be given as follows :

Let an infinitesimal be defined as a sequence of ordinary real numbers which converges to zero. Such a null sequence

$$a_1, a_2 \ldots a_n, \ldots \text{ or } (a_n)$$

being denoted by a single symbol a, we define the sum of two infinitesimals $a+b$ to be the sequence (a_n+b_n) and the product of an infinitesimal a by another infinitesimal b or by a finite number c to be the sequence (a_nb_n) or the sequence (ca_n).

Clearly, if N is any number, for all values of n greater than some value n_0, $|\, Na_n \,| < 1$, and in this sense, $|\, Na \,| < 1$, for all N.

Similarly we can define that $a = b$, if and only if the sequence (a_n/b_n) converges to unity. This implies that $[(a_n - b_n)/b_n] \to 0$, *i.e.*, that $|\, N(a_n - b_n) \,| < |\, b_n \,|$ for all $n > n_0$, and in this sense

$$|\, N(a-b) \,| < |\, b \,|.$$

It is now clear that null sequences are entities which satisfy the definition of infinitesimals and which give arithmetic significance to the extended definition of equality. This is sufficient to show that the preceding theory of infinitesimals is self-consistent.

IV. *Advantages of Theory.*

The advantages of the theory may be grouped under three headings : historical, practical and pedagogical.

1. *Historical.* I suppose we all feel very keenly that a great deal is missed by students from the simple fact that we have so little time in teaching calculus to give them any proper historical background. The manner in which I suggest the subject might be developed is the historical method in which it was developed by the earliest pioneers, and there are certain advantages in retracing in one's mind the historical line of development of the subject. One obvious advantage is that one realises how naturally some of the principal topics in the subject arise for discussion. Of course the disadvantage of historical treatment is that one is tempted to make all the mistakes made by one's ancestors. But a critical appreciation enables one to avoid that snare.

2. *Practical.* For engineers and physicists the calculus is simply a mere means of calculation ; they have absolutely no interest in its theoretical foundations. So long as we use watches without knowing how they work, we cannot condemn engineers who use the calculus without a study of its theory. Now the development of the calculus based on the concept of differentials avoids the theory of limiting processes which is the outstanding difficulty of the "practical" student, and should be of great value and benefit to such students.

3. *Pedagogic.* Even for professed students of mathematics I feel that it is of considerable advantage for them to acquire the technique of the calculus as rapidly as possible. It is a great help that they should have a certain technical facility with the subject before they sit down to consider theoretical questions—even those of fundamental importance. The real force and significance of the fundamental existence theorems is only properly appreciated by men who know something about how the theorem works in practice.

Disadvantages of the Theory.

There are, I think, two possible disadvantages of the theory. You will doubtless think of many more, but two inevitably suggest themselves.

First, the notion of equivalence, or the extension of the concept of equality. This notion, I am afraid, does suggest that the results are only approximate, and, as everyone knows, this is one of the commonest difficulties met by teachers of the calculus. Students *will* go away with the idea that, after all, the results are not exact but merely approximate, and that difficulty may appear to be aggravated by this method of treatment. I can only plead in extenuation that the concept of equivalence, or the extended definition of equality, is really necessary. Without this extended definition it is, in general, false to say that $df(x) = f'(x)\,dx$, *e.g.*, $d(x^2) = 2x\,dx + (dx)^2$, and we are justified in ignoring the $(dx)^2$ only by the extension of the definition of equality. With this extension

$$2x\,dx + (dx)^2 = 2x\,dx,$$

because the difference of the infinitesimals on either side of this equation is an infinitesimal of a higher order.

But there is a second disadvantage, and this is the main point which Klein urges against all those who dabble in infinitesimals (and their name is legion !). Klein pointed out that the real problem is to prove the first mean value theorem. I have not proved it from my axioms ; neither have I proved that it cannot be proved from these axioms, although I suspect that this is the case. Of course a great deal of calculus can be done without making use of the mean value theorem, but if it is required to introduce it, it seems to be necessary to introduce another axiom. (You will note that my method of simplifying the calculus is that whenever I find anything very difficult to prove, I just make it an axiom !)

What can be proved fairly simply is the fundamental result for the area $A(h)$ bounded by a continuous curve, $y=f(x)$, the coordinate axes, and the ordinate $x=h$. Clearly, $A(h+dh)-A(h)$ is the area under the curve between the ordinates $x=h$ and $x=h+dh$, and, for all values of x in this range,

$$f(x)=f(h),$$

by the definition of continuity given above. Hence

$$dA=f(h)\,dh.$$

V. *Definition of Circular and Exponential Functions.*

In order to make this a really workable programme for elementary students, it is necessary to show that it is applicable to all the elementary functions. There is no difficulty whatever with polynomials or circular functions, though a certain difficulty does arise in connection with the exponential function. Owing to our self-denying ordinance that we will not employ the theory of limits we have to modify the classical theory of the exponential function. Just as one can make the theory of circular function depend upon the circle supposed to be constructed by compass, so one can make the theory of exponential function depend upon the equiangular spiral. All that is necessary is to devise a method of constructing the equiangular spiral.

Perhaps the simplest method is by means of the " spiral compass " which consists of a straight rod, pivoted at one end O, and fitted with a sliding carriage which supports a wheel, which makes a fixed angle α with the rod. As the rod rotates the edge of the wheel bites into the paper and traces the equiangular spiral with the polar equation

$$r=A\,\exp\,(\theta\cot\alpha).$$

If the angle α is set at 45° and the initial distance, A, of the wheel from O is made the unit of length, the equation becomes

$$r=\exp\,\theta,$$

and the curve provides a geometrical definition of the exponential function, from which it is easily proved that

$$d\exp\,\theta/d\theta=\exp\,\theta.$$

I put this theory forward in quite a tentative way, in the hope that it may be of some use to those of you who are engaged in teaching the calculus to elementary students. My object is not to increase the difficulties of the subject, but to smooth the path of the beginner.

The question of the practical utility of this method can only be settled by experience and by experiment. The experiment of teaching calculus in this way will, I think, be one of great interest, and it belongs to the type of pedagogic experiment which is entirely justified. As to what the results would be, I cannot say, but I feel that if the method were successful it would revolutionise the teaching of calculus. In conclusion, may I express the hope that there will be a discussion and that someone will be so generous as to say a few kind words in my support.

Discussion

The President said he found it rather difficult to say anything on the paper. He was, in fact, rather near the end of his teaching days. Five years ago he handed over the top work at Harrow to a very competent man whom the Association had lately elected Treasurer. Last autumn he handed over the post-certificate work to two other men, one of whom had been working at it with him since he handed over the top work and he was devoting his own time to more elementary things. But he had had a great deal of experience of teaching the beginnings of calculus.

He had been delighted to hear Professor Temple say he was prepared to assume existence-theorems where he had difficulties over proving things. One of the battles that the speaker had had to fight over a good many things in teaching mathematics had been that the highbrows had wanted others not to play about with, for example, complex numbers until they could be put on a sound philosophical basis. But Professor Temple had said several times over that he thought—and the President agreed most heartily and was glad to have the Professor's support for what he felt—that it was possible to argue better about the philosophical basis of things when one had some practical utility in using them. In fact the grammar of a language should come after one had made some acquaintance with the language and not entirely before.

He feared that what Professor Temple had said with regard to differentials had made him feel that the whole question of differentials was really not a school but a university subject. Of course when he had the paper before him and could study more closely how Professor Temple dealt with various topics, he might change his mind and spend a year working on those lines. At the moment, however, his feeling was that differentials did not belong to the school course. He preferred the limit way of dealing with a differential coefficient or derivative. But when one introduced the integral notation, was one dealing with differentials? He would never allow a boy to use the integral sign until he had the idea of integration as a

summation,—or, rather, as the limit of a summation. Then, to take an example,

$$\int_a^b (x^2+3)^{20} \,.\, 2x \,.\, dx = \lim_{\Delta x \to 0} \sum_a^b (x^2+3)^{20} \,.\, 2x \,.\, \Delta x.$$

Now,
$$\frac{\Delta x^2}{\Delta x} \simeq \frac{dx^2}{dx} = 2x.$$

Thus
$$\Delta x^2 \simeq 2x \,.\, \Delta x$$

and so
$$\int_a^b (x^2+3)^{20} \,.\, 2x \,.\, dx = \lim_{\Delta x \to 0} \sum_a^b (x^2+3)^{20} \,.\, \Delta x^2$$
$$= \int_a^b (x^2+3)^{20} \,.\, dx^2.$$

From this it is easy to deduce that, as $dx^2/dx = 2x$, *under the integral sign* instead of $2x \,.\, dx$ we may write dx^2 ; and this can be generalised, always going back to the Δ stage.

That had nothing to do with differentials, and his feeling was that there was no need to introduce differentials into the elementary school work. He feared Professor Temple would regard him as somewhat unsympathetic towards what he personally regarded as a more advanced subject, though he confessed he would like to see what Professor Temple had illustrated worked out and note how far it could be applied to elementary work. On the whole, however, he had old-fashioned prejudices in favour of what he had been teaching for nearly thirty-seven years.

Mr. M. P. Meshenberg (Tiffin's) said he had been at a loss during Professor Temple's paper to decide just how honest his appearance of honesty was. Professor Temple had taken great pains to tell his audience exactly where he saw the flaws in his method but, as some were tempted to do with their pupils, he carefully abstained from letting out of the bag the biggest cat of all. The chief defect was in Professor Temple's assumption that he was evading the method of limits.

Then Professor Temple was keen on the historical justification, and the speaker felt sure it would appeal to Professor Temple to be reminded of what he was, no doubt, aware, that he had a predecessor on that road, Lagrange, who published a treatise on the calculus in which he promised to evade all appeal to limits and to use the method of expansions from the beginning. He was followed in England by Hall, a predecessor of Todhunter, who also used Lagrange's method of expansions. And yet the method of expansions was ousted and the speaker felt that the method of infinitesimals, as outlined by Professor Temple, would continue to be postponed until after the method of limits had been used. After all, how was a boy who had been made to feel the reality of the entity η going to be made to use it in the dry details of investigating derivatives or derived functions ?

Professor Temple allowed himself to slip by, by considering consecutive points. The speaker presumed the distance between Professor Temple's consecutive points would be his infinitesimal, but the distance would look somewhat different from that entity η which was a single member formed of the complex pair Professor Temple had in mind.

Professor Temple mentioned the fact that students would go away feeling that the results were approximations. That was obviously one of the chief drawbacks. That drawback was aggravated by another: the fact that things were not to be equal but equivalent. But, after all, the derivative of x^2 was not equivalent to but was precisely equal to $2x$ and, further, he denied that the differential of x^2 was anything but $2x\,dx$. De La Vallée Poussin, who had been responsible for a good deal of misrepresentation on the subject, drew a very fine and subtle distinction between what he called, in French, the *dérivé* and the *coefficient différentiel*. The speaker supposed one would render it in English by drawing a distinction between the derived function and the differential coefficient. But Professor Temple's use of equivalence instead of equalities in the idea of differentials was based upon a widely different notion. The speaker quite agreed that considering some of the fantastic gymnastics the procedure to the limit might sometimes perform one was obliged to introduce a symbol other than the symbol of pure equality. There was no doubt that the use of equivalence would aggravate the idea of the approximate character of the work, and on that score alone Mr. Meshenberg thought that for beginners it would stand condemned. He thought it more natural to make a boy appreciate the actual thing that he could see, steadily increasing the degree of approximation to something, than to visualise a peculiar entity symbolised by a sequence, and then, having been introduced to a sequence, he was to abolish the idea of the sequence and to replace it by a single letter η. It was an attempt to make a boy at that stage use a single letter for a multiplicity of equivalents.

But having said so much, Mr. Meshenberg added that he could not resist Professor Temple's pathetic appeal for a few kind words. He confessed that he proposed to try Professor Temple's method next year. There was much that appealed to one, if only as a test of one's own self-control and one's ability to impose upon unripe students! He wondered how long he could make his boys really believe that they did know those entities η and that $|N\eta|<1$ meant something to them. For how long could he make them believe that, before they began asking some rather awkward questions? One got a bright spark occasionally asking very awkward questions. If he was obliged sooner or later to take that boy down to the null sequences he feared the gaff would be blown, because he would be obliged in sheer honesty to show the boy how some things might approach some very queer limits as well as the limit zero. Would Professor Temple explain how one was to get over the little underground cellar to which he had relegated all the limit work, which he did

really use ; how he was going to get over the difficulty of things which had a multiplicity of limits ? They all knew that sin $(1/x)$ as $x \to 0$ could be made to produce any chosen number between ± 1 they liked. It was, indeed, amusing to make a number of young boys calculate the limit of sin $(1/x)$ as $x \to 0$ and make it come out to be, say, ·76 ; and the boys could do that quite readily after a little while. Then the next day they could be asked to make it come out at $-$·76 ; and the day after they could be asked to make it come out, perhaps, as 1. But by that time the boys would have given their teacher up as the confirmed wangler which they had suspected him to be all the time !

Mr. M. Black (Royal Grammar School, Newcastle-on-Tyne), spoke as an unrepentant highbrow. He did not wish to say that one should give a full philosophical discussion of all entities used in mathematics. Perhaps nobody in England had sufficient knowledge or time for that. In teaching logarithms, for example, he himself taught boys to use log tables before they had much conception of what they were about. Nevertheless it would be a mistake not to have an intelligible explanation ready in the background. If it was necessary to teach certain operations mechanically, they should do so unashamedly and say to their pupils : " This is a machine for calculations and the arguments are fudge, but it is useful and therefore we do it ; if you want to know more about it, here are the references. I am prepared to explain and defend the whole process." The use, for example, of fractional indices before boys had a clear understanding of them was defensible, because the ideas involved were plausible and followed naturally from previous work. But the use of ideas which were even superficially contradictory was not justifiable. To teach calculus in that way to a pupil beginning the subject would be to go back to the bad old days when Higher Mathematics seemed a form of higher magic. It would, in the speaker's view, be a great mistake in teaching if they were to reject the methods of limits which could, after all, be given a fairly easy explanation, in order to return to the practice of introducing entities which could not be defended without a great deal more knowledge than they had.

Professor Temple, in replying, thanked the President for his kind and appreciative remarks. He had been glad to get from a teacher of such experience confirmation of his own necessarily much more immature views upon the advisability of postponing formal existence-theorems until after a certain technical facility had been obtained. He thanked the President also for the very apt illustration that grammar necessarily follows speech. He agreed that it was possible to develop the calculus without any reference to differentials, and that it was possible to justify the change of variable in an integral without any appeal to differentials. His object, of course, was to eliminate, as far as possible, the method of limiting processes, but he was rather careful to say, or at least to write in his notes—and he hoped that he had said it—that what he intended to do was to construct a theory which was formally independent of the theory of

limits. He was well aware that as soon as he came to jusitfy his definition of infinitesimals he did so by the use of convergent sequences, but that only appeared in the justification. In the first-year course there would be no justification whatever. The students would, he hoped, accept the axiom and work on that and the theory of limits would not appear. It would lurk in the background ready to be brought forward when he was challenged for justification of his axioms, and to prove that such entities as differentials existed; *i.e.* when he was challenged to prove that he could create such entities when required.

In reply to Mr. Meshenberg, he said that he was very happy to be condemned with Lagrange. *His* method of using a power-series was, of course, also formally independent of the theory of limits, and required the justification provided by Abel's famous paper on convergence. Professor Temple added that he had not yet shot his last bolt. He had other methods of reaching the calculus without the theory of limits at all. If the Association could stand the strain he would venture to unburden himself on another occasion. They were purely algebraic methods and there was no mention of the theory of limits.

(In reply to Mr. Meshenberg's criticisms the treatment of "equivalence" has been recast. The revised version is printed above.) He much regretted that he had not heard the opinion of those who used differentials in Applied Mathematics, because few, if any, textbooks on Applied Mathematics did not use differentials.

Lastly, he wished to issue a challenge to the speakers, some of whom had spoken as if they were able to present the calculus free from all that was abstract and arbitrary. The President had spoken as if he was able to prove that the area under a curve satisfied the equation

$$dA/dx = f(x),$$

but he challenged those present to construct such a proof using only elementary concepts. If one had lurking in the background all sorts of abstract theorems relating to upper and lower bounds, he thought that his modest axiom $|N\eta| < 1$ was comparatively innocuous.

1052. I discovered only subsequently that the concept of the transcendent (or transcendental) function occurs also in higher mathematics, and that it is actually the name of the function dealing with real and imaginary numbers.—C. G. Jung, footnote to p. 83 of *Two essays on analytical psychology*, translated by H. G. and C. F. Baynes. [Per Mr. G. B. Ehrenborg.]

1053. The arithmetic was elementary, and she relates that "we never touched vulgar fractions; there was an impression that they were not quite nice"..... But she admits that "from first to last" algebra remained for her "an impenetrable mystery".—The *Times*, November 12, 1935; obituary notice of Miss Frances Gray, first High Mistress of St. Paul's Girls' School. [Per Rev. T. W. Ward Hill.]

NAPIER'S EDUCATION—A SPECULATION.

By Alex. Inglis.

It has been shown * that the evolution of logarithms involves consideration of Napier's knowledge of Greek; this in turn is connected with his "educational and cultural environment". He certainly studied abroad for some years but where he spent the time is largely a matter for speculation. Now even speculation must be based on some form of reasoning, therefore let it be taken as a hypothesis that as his mathematical equipment was considerably in advance of that of his contemporaries, this equipment must have been the result of ideas planted by his teachers. Consequently an explanation of Napier's travels may be made by considering his work in mathematics and by discovering similarities in the works of mathematicians who were in Europe during the years of his residence abroad. It must be remembered also that it was a time of acute religious differences, and that he belonged to a family that was ardently Protestant in its sympathies. His own Protestantism is quite openly expressed in his work on the Apocalypse. Therefore these speculations are based on the "historico-religious" background of the period and on Napier's mathematical attainments.

It was a period of unrest and disorder in Scotland which was mainly due to the state of unstable equilibrium produced by the Reformation affecting all the activities of the time. Politically Scotland was in alliance with France, but in her true interests "Flanders was more valuable to her; . . . in prosaic fashion the intercourse was productive of unmixed good in showing the way to an intelligent cultivation of the resources of the country". † Most of the trade was with the Low Countries and the commercial treaty was renewed in 1545. While the Catholic party was strong in its support of the French alliance, the Protestants were bitterly opposed to it and were successful in bringing it to an end by the Treaty of Edinburgh in 1560. "The Scottish scholar, to his loss, now forced himself to be content with such learning as he could get in St. Andrews or Glasgow or Aberdeen. The cultured Scotsman had no longer, as it were, one foot in Paris and one in Edinburgh; his horizon was contracting." ‡

John Napier certainly matriculated at St. Salvator's College, St. Andrews, in 1563, for Mark Napier found a record of it in Edinburgh. According to Hume Brown, the length of time Napier spent at St. Andrews is uncertain and his further movements are unknown. § Mark Napier states quite definitely that he left St. Andrews for the University of Paris in 1564. ‖ There are many arguments against this, the chief being that it is unlikely that a Protestant father of

* *Mathematical Gazette*, July 1935, 192-205.

† P. Hume Brown, *History of Scotland*, i. 344.

‡ R. L. Mackie, *A Short History of Scotland*, 238.

§ *Napier Memorial Volume*, 37.

‖ *Memoirs*, 106.

the time would send his son to be educated at a stronghold of Catholicism, particularly as one of the reasons for removing him from St. Andrews may have been his friendship " with a certain Gentleman, a Papist ".* Adam Bothwell, Bishop of Orkney, had written in December 1560: " send your son John to the schools; over to France or Flanders; for he can learn no good at home ". Bothwell was an influential man at the time and likely to be familiar with conditions at home and abroad, and he advised: " send your son to Catholic France or Protestant Flanders; educationally both are equally good ". Therefore it seems quite likely that Napier went to Protestant strongholds of Europe, and curiously enough the " Greek theory " shows that he probably spent some time at Geneva †—the Vatican of Protestantism!

An examination of the *De Arte Logistica* shows that Napier had a considerable knowledge of algebra. In it he used the " Pascal Triangle " for the extraction of roots; he carried his calculations only as far as fourth powers but indicated how the higher roots may be obtained. Michael Stifel, 1486-1567, also used the " triangle " in his *Arithmetica Integra*, 1544, and according to Hutton he used it for the extraction of roots.‡ Another mathematician of the time familiar with the " triangle " was Johann Scheubel, professor of mathematics at the University of Tübingen, 1550-1570. He was " scholarly, heavy, balanced, dignified ",§ in fact a type likely to appeal to a Scot. In his *De Numeris et Diversis Rationibus*, 1545, he used the " triangle " to extract roots as high as the twenty-fourth. A further link between Scheubel and Napier is that while abroad the latter " studied the history of Arabic notation, which he traced to its Indian source ".‖ Now Scheubel left in manuscript a copy of Robert of Chester's translation of al-Khowarizmi's Algebra. He was obviously interested in the origins of algebra, for he possessed a copy of a work written in the twelfth century before printing was in existence, and which would therefore be difficult to procure. It is the first work bearing the word " algebra " in its title.¶ Incidentally, Scheubel is the first modern mathematician to mention Diophantus, which he does in his *Algebra Compendiosa Facilisque Descriptio*, 1552.** Simon Stevin, 1548-1620, is also credited with having developed the use of the " triangle " and of having extracted roots by its aid. Stevin and Napier are also connected through their pioneer work on decimal notation. Decimal fractions were definitely propounded for the first time in Stevin's *Arithmetic*, published in 1585. Napier's *De Arte Logistica* was written probably soon after his return home in 1571, and in it there is a notation for decimal fractions.

Stifel was an Augustine monk who followed Luther—he was

* The preface to the *Plaine Discovery*.

† *Mathematical Gazette*, July 1935, 198.

‡ *Mathematical and Philosophical Dictionary*.

§ D. E. Smith, *History of Mathematics*, i. 329.

‖ H. W. Turnbull, *The Great Mathematicians*, 61.

¶ D. E. Smith, i. 203. ** Hutton, i. 79.

certainly a Protestant; Stevin was an official of Prince Maurice of Orange—he was probably a Protestant; all that is known of Scheubel is that he was a professor of mathematics. The connections between the mathematical work of these three men and that of Napier is rather striking, to say the least, and it is natural to wonder whether he came into contact with Stifel and Scheubel while studying in Germany. Stevin he may have met almost anywhere, for Stevin's official duties caused him to travel about Europe. Consequently it may be argued thus: Napier was sent abroad with letters of introduction probably to Protestants, and hence most likely to the Low Countries as there was a definite tie between them and Scotland. Through these contacts later he connected with Protestants in Germany and so met Stifel and Stevin. The case of Scheubel presents rather more difficulty. But both Stifel and Stevin knew the coefficients of $(1+x)^n$, where n is positive, integral and no more than 17. Napier's interest would be aroused, and he might hear of the man who could extract roots as high as the twenty-fourth by means of similar expressions. Thus he came to know Scheubel, studied at Tübingen and so learnt something of the origins of algebra.

Some years ago a most interesting suggestion was made by Professor H. Stanley Allen of the University of St. Andrews concerning the evolution of logarithms. Professor Allen suggested that there may have been some connection between Napier's work on spherical trigonometry and this discovery; further, that Napier's interest in the geometry of the sphere was fostered through consideration of problems in navigation, such as the sailing of a vessel along a rhumb line. This suggestion certainly agrees with the speculations above, for it presumes that Napier must have learnt something of navigation somewhere. At that time Scotland was not a maritime nation, in fact the longest voyage made by Scottish vessels was merely across the North Sea—little more than a coasting journey. The sea-going adventurers of England, Spain, Portugal and Holland were all endeavouring to find fame and fortune through long voyages to little known parts of the world; therefore, in these countries navigation problems would be frequently discussed. Did Napier study in any of these? Spain and Portugal can be definitely omitted on religious grounds. If he had spent any time in England his name would appear most probably in the records of Oxford or Cambridge. It does not, and even if by some chance he was elsewhere in England it is unlikely that some record would not be found. Therefore Holland is the remaining country; it was largely Protestant, it was associated commercially with Scotland, and it was interested in navigation.

ALEX. INGLIS.

1054. Codex de Mathematicis et Maleficiis.

Mathematicus was the name given to astrologers: as a law of Diocletian put it, "artem geometriae disci atque exerceri publice interest. Ars autem mathematica damnabilis est et interdicta omnino".—W. E. H. Lecky, *Rationalism in Europe*, p. 41 (1910). [Per Prof. H. G. Forder.]

PRESENTATION TO MR. PENDLEBURY.

At the Annual Meeting in January, a report of which is included in the February number of the *Gazette*, Mr. Siddons gave an account of the disposal of the sum subscribed for a present to Mr. Pendlebury on his retirement. He also read a message from Mr. Pendlebury, who was unfortunately unable to be present.

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The Council of the Mathematical Association also voted a contribution of £5 5s.

BUREAU FOR THE SOLUTION OF PROBLEMS.

THIS is under the direction of Mr. A. S. Gosset Tanner, M.A., Derby School, Derby, to whom all inquiries should be addressed, accompanied by a stamped and addressed envelope for the reply. Applicants, who must be members of the Mathematical Association, should wherever possible state the source of their problems and the names and authors of the textbooks on the subject which they possess. As a general rule the questions submitted should not be beyond the standard of University Scholarship Examinations. Whenever questions from the Cambridge Scholarship volumes are sent, it will not be necessary to copy out the question in full, but only to send the reference, *i.e.* volume, page, and number. The names of those sending the questions will not be published.

CORRESPONDENCE.

ALGEBRAIC EQUATIONS WITH COMPLEX COEFFICIENTS

To the Editor of the *Mathematical Gazette.*

DEAR SIR,—Can any reader give me, or refer me to, a practical method of finding numerically the roots of an algebraic equation with *complex* coefficients? This may appear to be an academic question, but actually I have recently met it in connexion with a piece of practical work. For degrees up to 4 ordinary methods of solution will apply, square- or cube-roots of $\alpha+i\beta$ being found trigonometrically; but there are no similar methods for higher degrees. The presence of complex coefficients means that there are roots of the form $\alpha+i\beta$ unaccompanied by the conjugate $\alpha-i\beta$; hence it seems to me—in spite of a statement to the contrary which I have read in a French memoir—that the ordinary root-squaring method is inapplicable since we cannot separate the moduli from the periodically-fluctuating arguments. The only method I am able to suggest is the following: it is admittedly very operose, involving the solution of an equation of doubled degree.

Collect imaginary terms and write the equation in the form

$$P(x)+iQ(x)=0,$$

where $P(x)$ and $Q(x)$ are polynomials. Multiplying by $P(x)-iQ(x)$ we get

$$[P(x)]^2+[Q(x)]^2=0,$$

an equation of doubled degree with real coefficients, having conjugate pairs of roots of the form $\alpha\pm i\beta$. These may be found by the root-squaring method as improved and completed by Brodetsky and Smeal, *Proc. Camb. Math. Soc.*, vol. xxii, p. 83 [1924]. These roots will include those we want and also their unwanted conjugates. The latter can be rejected by substitution and trial: or usually it would be practicable, by a tentative process, to select the roots whose imaginary parts add up to the negative of the imaginary part of the second term in the original equation.

Your obedient servant,

G. J. LIDSTONE.

1055. James I also paid a visit to Tycho Brahe on the island of Hveen, which gave him indescribable pleasure: he believed that in his company he fathomed the marvels of the universe, and lauded the astronomer in spirited Latin verse as the friend of Urania, and as the master of the starry world.—L. von Ranke, *History of England*, vol. i (Oxford, 1875), p. 367.

1056. FINGER MULTIPLICATION.

Every lady of the land
Has twenty nails upon each hand
Five and twenty hands and feet
This is true without deceit.

—W. Kerr, *The English Apprentice*, p. 190. [Per Mr. V. Naylor.]

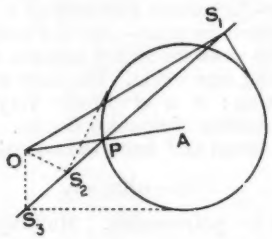
MATHEMATICAL NOTES.

1183. *Bieberbach's trisection.*

In an article in *Crelle* (vol. 167, p. 142, 1932), Bieberbach gives the following theorem, which leads to a simple mechanical trisection of an angle :

A line is drawn through a fixed point P of a circle, with unit radius and centre A, to make a given angle θ with the radius PA. If S is a variable point of this line, and O is the reflection of A in P, the line through S perpendicular to SO is a tangent to the circle if, and only if, $PS = 2\cos\frac{1}{3}\theta$.

The direct verification of this theorem is of some interest as an illustration of avoiding the steps that the beginner regards as



obvious. Taking the origin at P, not at the centre of the circle, we have the point S as $(r\cos\theta, r\sin\theta)$, and the condition to be expressed is that the line

$$(1+r\cos\theta)(x-r\cos\theta)+r\sin\theta(y-r\sin\theta)=0$$

touches the circle

$$x(x-2)+y^2=0.$$

Not eliminating one variable but using the equation of the line to make the equation of the circle homogeneous, we have only to express that the roots of the homogeneous equation

$$r(r+\cos\theta)(x^2+y^2)=2x\{x(1+r\cos\theta)+yr\sin\theta\}$$

are equal, whence r, if not zero, satisfies the equation

$$(r+\cos\theta)(r^2-r\cos\theta-2)=r(1-\cos^2\theta),$$

that is,

$$r^3-3r=2\cos\theta.$$

E. H. N.

1184. *A geometrical transformation.**

1. Let O be a fixed point in a plane, M a variable point, M' the foot of the perpendicular from M on the line got by turning OM round O through a (constant) angle α. Denote by T the transformation replacing M by M' ; we write $M'=TM$, $M=T^{-1}M'$. If M

* Some of the results in this note were given by me in *Nouvelles Annales*, 1922-3, pp. 109-12. The proofs have been shortened and simplified here.

describes a curve (M), M' describes the curve $(M')=T(M)$. If we need to specify the angle α, we can write T_α for T.

The curves (M) and (M') are clearly similar. If Mt and $M't'$ are the tangents, the angles OMt and $OM't'$ are equal, and hence, if Mt and $M't'$ meet at P, the points O, M, M', P are concyclic. Since the angle $OM'M$ is right, it follows that the angle OPM is also right. Hence *the tangents to the curves $T(M)$, at the points corresponding to a given point M, meet at the corresponding point on the pedal curve of (M).*

Again, the points $T^{-1}M$ lie on the perpendicular to OM at M. This line envelops the negative pedal of (M).

2. There will be a certain number (depending on the form of the curve and the position of O) of points on (M) where the angle OMt has the value $\frac{1}{2}\pi-\alpha$, α being any given angle. The corresponding point $M'=T_\alpha M$ will coincide with the foot of the perpendicular from O on the tangent Mt. Further, (M') and the pedal of (M) will have the same tangent at the point; hence *the envelope of the curves $T(M)$, for different values of α, is the pedal of (M).*

The line MM' envelops the negative pedal of (M'). In the special case just considered, where MM' touches (M) at M, M is also the point of contact of MM' with the negative pedal, since the angles $OM't'$ and OMM' are equal. Hence, dropping the accents, *the envelope of the curves $T^{-1}(M)$ is the negative pedal of (M).*

3. Let A and B be corresponding points on any two directly similar curves, and let O be the centre of similitude. Let M be the intersection of the perpendiculars to OA and OB at A and B respectively, and let N be the foot of the perpendicular from O on AB. Since the triangle OAB is of constant species, all the other triangles in the figure will have the same property, and the curves (A) and (B) will both be $T(M)$ and $T^{-1}(N)$. The four curves are similar, hence: *The locus of the intersection of corresponding tangents to two directly similar curves is the pedal, with respect to the centre of similitude, of a curve similar to the two and with the same centre of similitude.* And: *The join of corresponding points envelops the negative pedal of another curve of the same type.* Both envelope and locus will touch the two given curves in a certain number of points.

4. Two circles are directly similar in an infinity of ways, and we can make pairs of tangents correspond which contain any given constant angle. Hence the locus of the point of intersection of such a pair of tangents is a limaçon bitangent to the two circles, and the envelope of the join of corresponding points is a conic also bitangent to them. If the angle between the tangents is equal to the angle between the circles, so that one of the two points of intersection of the circles corresponds to itself, the locus becomes a cardioid and the envelope reduces to the other point of intersection of the circles. (This theorem is, of course, well known.)

Another simple example is that of two parabolas, or two similar conics, having a common focus.

M. F. EGAN.

1185. *A group of coaxial circles; generalization of the orthopole-theorem.*

Let ABC be a triangle, P_a, P_b, P_c and Q_a, Q_b, Q_c the points where BC, CA, AB meet the joins from A, B, C to a point P and the polar of P for the triangle; if P_a', P_b', P_c' and Q_a', Q_b', Q_c' are the projections of P_a, P_b, P_c and Q_a, Q_b, Q_c on a straight line Δ, the circles Γ_a, Γ_b, Γ_c having their centres on BC, CA, AB and passing respectively through P_a' and Q_a', P_b' and Q_b', P_c' and Q_c' are coaxial.

Let C_a, C_b, C_c be the circles having their centres at A, B, C and tangent to Δ at A', B', C'. It is well known that the locus of a point whose powers for two given circles are in a given ratio is a circle: therefore Γ_b is the locus of the points whose powers for C_c and C_a are in the ratio

$$\overline{P_b'C'}^2 : \overline{P_b'A'}^2 = \overline{Q_b'C'}^2 : \overline{Q_b'A'}^2.$$

But when the point considered on Γ_b belongs also to Γ_c, its powers for C_a and C_b are in the ratio

$$\overline{P_c'A'}^2 : \overline{P_c'B'}^2 = \overline{Q_c'A'}^2 : \overline{Q_c'B'}^2.$$

Hence the powers of the intersections of Γ_b and Γ_c for C_c and C_b are in the ratio

$$\begin{aligned}&(\overline{P_b'C'}^2 : \overline{P_b'A'}^2) \times (\overline{P_c'A'}^2 : \overline{P_c'B'}^2)\\ &= (\overline{P_bC}^2 : \overline{P_bA}^2) \times (\overline{P_cA}^2 : \overline{P_cB}^2)\\ &= \overline{P_aC}^2 : \overline{P_aB}^2 = \overline{P_a'C'}^2 : \overline{P_a'B'}^2;\end{aligned}$$

therefore these intersections are also on the circle Γ_a, and this proves the stated theorem.

When P is the centroid of ABC, $Q_aQ_bQ_c$ is at infinity, and we have the following property:

If A_m', B_m', C_m' are the projections of the mid-points of BC, CA, AB on a straight line Δ, the perpendiculars from A_m', B_m', C_m' on BC, CA, AB are concurrent.

This is the orthopole-theorem applied to the medial triangle of ABC.

R. GOORMAGHTIGH.

1186. *Integers expressible as the sum of two rational cubes.*

Prof. A. Lodge's note on this subject in the *Gazette*, 1935, p. 219, contains two questions: (i) when an integer is the sum of two rational cubes, to find an infinite number of similar expressions for the same integer; (ii) to find such integers.

The first question has already been considered by Fermat (see *Œuvres*, vol. iii, Additions et remarques sur Diophante); his formula

$$a^3+b^3 = \left[\frac{a(a^2+2b^2)}{a^3-b^3}\right]^3 - \left[\frac{b(2a^2+b^2)}{a^3-b^3}\right]^3$$

gives sets of cubic splits of the kind mentioned by Prof. Lodge.

The same formula will also be found in Prestet's *Nouveaux*

Eléments de Mathématiques, 1689, p. 260. Legendre has generalized the method for integers of the form x^3+cy^3.

Another formula has been given by Werebrussow (*Intermédiaire des Mathématiciens*, 1905, p. 268); new splits are obtained when two of them are known:

$$a^3+b^3=\alpha^3+\beta^3=\frac{(a^2\beta-\alpha^2 b)^3}{(\alpha\beta-ab)^3}+\frac{(\alpha b^2-a\beta^2)^3}{(\alpha\beta-ab)^3}$$
$$=\frac{(\beta b^2-\alpha^2 a)^3}{(\alpha\beta-ab)^3}+\frac{(\alpha a^2-\beta^2 b)^3}{(\alpha\beta-ab)^3}.$$

On the other hand, Lenhart (1787-1840) has published a table of integers N expressible in the considered form; the following values for $N<300$ are extracted from this table: 6, 7, 9, 12, 13, 15, 19, 20, 26, 28, 30, 33, 35, 37, 43, 62, 63, 65, 70, 84, 86, 89, 91, 98, 105, 107, 110, 117, 124, 126, 133, 134, 143, 153, 163, 171, 180, 182, 183, 201, 203, 209, 217, 218, 219, 223, 254, 259, 273, 278, 279, 287, 289.

Lamé (*Comptes-rendus de l'Académie des Sciences de Paris*, 1865) has also given, amongst others contained in Lenhart's Table, the value $N=215$; in *Sphinx-Œdipe*, 1916, p. 94, Bastien finds $N=53$, 103, and Gérardin $N=75$, 78, 85, 87, 164, 275.

But an article by Pépin (*Journal de Mathématiques pures et appliquées*, 1870, pp. 217-236) contains several theorems on the possibility of the problem for a given integer N. For instance, it is impossible when, p being prime of the form $18m+5$, q prime of the form $18m+11$, and n any positive integer, the considered integer N is p, p^2, q, q^2, $2p$, $2q^2$, $4p^2$, $4q$, $9p$, $9q$, $9p^2$, $9q^2$, $5q$, $5p^2$, $25p$, $25q^2$ or p^n, q^n, $2p^{3n+1}$, $2q^{3n+2}$, $4p^{3n+2}$, $4q^{3n+1}$, $9p^{3n+1}$, $9q^{3n+1}$, $9p^{3n+2}$, $9q^{3n+2}$, $5p^{3n+2}$, $5q^{3n+1}$, $25p^{3n+1}$, $25q^{3n+2}$.

Pépin shows also that the problem is possible when N has the form $h(h+1)$, a general theorem proving the error of Legendre, who thought that 6 could not be expressed as the sum of two rational cubes.

More generally, the formula of Desboves (*Comptes-rendus*, 1878),

$$hk(h+k)[3(h^2+hk+k^2)]^3$$
$$=[h^3-k^3+3hk(2h+k)]^3+[k^3-h^3+3hk(2k+h)]^3,$$

shows that the problem can be solved when N has the form $hk(h+k)$.

Other indications on the subject are to be found in the works of J. J. Sylvester and Ed. Lucas. R. GOORMAGHTIGH.

1187. *On Note* 1152.

Professor Lodge asks in effect for a method of finding integers expressible as the sum or difference of two cubes in more than one way. The following process may be of interest:

$$3^3+4^3+5^3=6^3.$$

Write this in the form

$$3^3-(-4)^3=6^3-5^3.$$

Then 3 is a solution of the equation

$$x^3-(x-7)^3=(x+3)^3-(x+2)^3.$$

This equation reduces to a quadratic and has therefore another solution, which must be rational. It is found to be 6. Hence we discover that

$$6^3+1^3=9^3-8^3.$$

This identity can be remodelled so as to give further solutions. For example,

$$1^3-(-8)^3=9^3-6^3,$$

through $$x^3-(x-9)^3=(x+8)^3-(x+5)^3,$$

gives $$19^3-10^3=27^3-24^3.$$

So far as I can see the process and the progeny can be continued indefinitely. JAMES STRACHAN.

1188. *An elementary collision problem.*

The following problem provides a good example of the algebra that arises in collision problems and may be of interest to anyone with a few hours to spare. It may at first sight appear to be very simple, but my use of the word " hours " is, I believe, correct.

Four exactly similar balls A_1, A_2, A_3, A_4 are at rest at equal distances apart in a smooth infinite horizontal groove. A_1 is then projected towards A_2 with velocity v. If e is the coefficient of restitution for any pair of balls, what is the subsequent behaviour of the system ?

It will be found that the value of e is important, and the different cases that arise are as follows :

I. $e\leqslant 3-2\sqrt{2}$.

The balls do not finally separate, *i.e.* no time t can be found such that any ball does not take part in a collision after time t. The velocity of each ball tends to become $v/4$.

II. $e>3-2\sqrt{2}$.

After a finite number of collisions, the balls all separate and do not meet again. If $N(1, 2)$ is the number of collisions between A_1 and A_2, etc., these numbers may be found as follows. Let θ be defined by $\cos\theta=(1-e)^2/4e$, $0<\theta<\frac{1}{2}\pi$, and let n be the positive integer given by $\dfrac{\pi}{n}\leqslant\theta<\dfrac{\pi}{n-1}$. Then if n is even, the numbers of collisions are

$$N(1, 2)=N(2, 3)=N(3, 4)=n-1,$$

and if n is odd, the numbers are

$$N(1, 2)=n,\quad N(2, 3)=n-1,\quad N(3, 4)=n-2.$$

The final state is particularly interesting when $\theta=\pi/n$. If n is even, A_1, A_2, A_3 finally have the same velocity $v(1-e^n)/4$, less than that of A_4, $v(1+3e^n)/4$. If n is odd, A_2, A_3, A_4 finally have the same velocity $v(1+e^n)/4$, greater than that of A_1, $v(1-3e^n)/4$.

It is easily seen that n decreases rapidly from infinity as e increases from the value $3-2\sqrt{2}$ or ·1716. Thus, when $e=\cdot172$, $n=38$, when $e=\cdot2$, $n=5$, and when $e\geqslant 2-\sqrt{3}=\cdot268$, $n=3$.

The above results are obtained under the assumption that consecutive collisions between A_2 and A_3 are separated by a collision between A_1 and A_2 and another between A_3 and A_4. This is most probably true, but I have been unable to prove it generally.

A. G. WALKER.

1189. *Group theory definitions in Webster's "New International Dictionary".*

Webster's *New International Dictionary*, second edition, 1934, contains a number of entries relating to group theory which do not appear in the earlier editions of this widely used work. Some of the definitions which appear in connection with these entries are so misleading that it seems desirable to note here needed modifications which may be of special interest to teachers of mathematics. Under the entry "dicyclic group" it is stated that such a group is generated by two elements a and b, in such a manner that every element is of the form a^x or ba^x, the element a satisfying the equation $a^{2m}=1$, and the element b having its square $=a^m$, where m is a positive integer.

While it is obviously true that every dicyclic group contains two such generating elements, where $2m$ is the order of a, it is not true that the given conditions are sufficient to define such a group. In fact, the number of distinct groups of order $4m$ which satisfy these conditions is 2^n, 2^{n+1}, or 2^{n+2}, n being the number of the distinct odd prime numbers which divide m, as m is an odd number, twice such a number, or divisible by 4, respectively. Only one of these groups in each case is dicyclic, and hence the number of the groups of a given order which are generated by two such operators but are not dicyclic may exceed any given fixed number. The infinite category of groups whose two generating elements satisfy these conditions is unusually interesting in view of the very elementary properties of these separate groups.

Under the entry "permutable" an almost meaningless statement appears, since a group is obviously permutable with itself, but the most serious inaccuracy relating to group theory appears under the entry "group" itself. This term is now so widely used in our educational literature that one might reasonably expect to find a satisfactory definition of it in a work which is advertised as "the foundation book of education". This is especially true since some of the satisfactory definitions are brief. The given definition is obviously satisfied by the three well-known operators 1, 2, 3, which are not associative but satisfy the following equations:

$$1\,1=1,\quad 3\,1=2,\quad 2\,2=2,\quad 1\,3=2,\quad 3\,3=3,$$
$$2\,1=3,\quad 1\,2=3,\quad 3\,2=1,\quad 2\,3=1.$$

What is perhaps the most striking in connection with this defini-

tion is the fact that reference is made to the entry "distribution", and under this entry there appears the statement that objects arranged in classes are called *parcels* when the order is indifferent and *groups* when it is essential. This statement appeared also in the earlier edition of this dictionary and hence it might at first appear to have stood the test of time. It is, however, absurd as regards mathematics and it is to be hoped that it will be eliminated in later editions. Cf. *School and Society*, volume 40, 1934, page 728. G. A. MILLER.

1190. *Special Integrals of Differential Equations.*

With regard to Professor Piaggio's criticism of my remarks in the May *Gazette* on Special Solutions, I have to express my regret that I misquoted his reasons for calling $z=0$ a special solution of the equation $p-q=2\sqrt{z}$. At the same time this does not affect the fact that the solution in question is a perfectly regular singular solution representing an envelope of the rationalised equation, and that most of the other examples of special solutions given in the textbooks are examples of the same type, the others being simply special forms of a complete or general integral. However, I admit that in other cases integrals more deserving the name of special may exist.

I think the misunderstanding has arisen in part from indefiniteness about the type of functions appearing in Lagrange's equation. Is it right, for instance, to call an equation such as the above, containing a two-valued function, a Lagrange equation ?

I shall look forward to a paper by Professor Piaggio on the subject. R. J. A. BARNARD.

1191. *The Remainder Theorem.*

The last lines on the Remainder Theorem in the *Algebra Report* (p. 36) read as follows :

"Why is it permissible in Method (A) to put $x=a$ after dividing by $x-a$, although division by zero is impossible ? "

Method (A) is the proof by using

$$(x-a)q(x)+r\equiv f(x). \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\text{(I)}$$

Only once has a boy asked me this question, and the boy was fresh from his Preparatory school. A boy with sufficient discernment to ask the question will be uneasy when he is asked to accept the explanation that (I) represents a multiplication, not a division. The identity is obtained by division (see explanation in the *Report*) and is to be applied to division.

Is division by zero really impossible, or has the teacher fallen a victim to a caution he instils into his class ? In heading his pupils off one error, has he himself fallen into another ?

Division by zero is impossible only if there is to be no remainder. Without this restriction, the division is possible ; for example, 2d. can be given 6 times out of 1s. and there is no remainder. "Nothing" can be given as many times as you like from a shilling and the shilling still remains. In other words, if 12 is divided by 0, the

quotient is any number whatever and the remainder is always 12. Similarly, if $f(x)$ is divided by $x-a$ and $x-a$ is zero, then the remainder is equal to $f(x)$, and, with the condition that $x=a$, it is $f(a)$.

The first line of p. 36 reads:

"The actual division of bx^3+mx^2+nx+p by $x-a$ is useful".

In this actual division the pupil will perceive that the complete remainder at any stage is equal to the dividend if a is substituted for x in each, and he will understand that this is bound to be so, since at each stage of division he will be subtracting a multiple of $x-a$—that is, he will be subtracting zero—if his divisor is zero.

F. C. Boon.

1192. *Sur les points de Feuerbach.*

M. N. M. Gibbins vient de donner la démonstration de son élève Plotnick d'un théorème de Purser, qui conduit à une démonstration simple du théorème de Feuerbach.* Voici quelques conséquences du raisonnement employé pour montrer que, dans un triangle, le cercle des neuf points est tangent aux quatre cercles tritangents.

Soient un triangle ABC ($BC=a$, $CA=b$, $AB=c$); A_1, B_1, C_1 les milieux des côtés BC, CA, AB; A', B', C' les pieds des hauteurs; H l'orthocentre; α, β, γ les points eulériens, (milieux des segments rectilignes AH, BH, CH); (O), O_g), (I), (I_a), (I_b), (I_c) les cercles circonscrit, des neuf points et tritangents, de rayons R, $\frac{1}{2}R$, r, r_a, r_b, r_c; D, E, F, D_a, ..., F_c les contacts des cercles tritangents sur BC, CA, AB; ϕ, ϕ_a, ϕ_b, ϕ_c les contacts du cercle (O_g) avec les cercles (I), (I_a), (I_b), (I_c), (points de Feuerbach).

1. Le point ϕ, par exemple, étant le *point-limite* du faisceau des cercles (O_g) et (I), on a

$$\left|\frac{A_1D}{A_1\phi}\right|^2=\left|\frac{b-c}{2A_1\phi}\right|^2=\left|\frac{O_gI}{O_g\phi}\right|=\left|\frac{R-2r}{R}\right|, \quad \text{.....(1)}$$

puis,

$$\left|\frac{A'D}{A'\phi}\right|^2=\left|\frac{(b-c)(p-a)}{a\,.\,A'\phi}\right|^2=\left|\frac{R-2r}{R}\right|, \quad \text{.....(2)}$$

enfin,

$$\left|\frac{\alpha\alpha'}{\alpha\phi}\right|^2=\left|\frac{r+r_a-2R}{2\phi\alpha}\right|^2=\left|\frac{R-2r}{R}\right|, \quad \text{.....(3)}$$

α', β', γ' étant les contacts des tangentes au cercle inscrit (I), issues des points α, β, γ.

Dès lors, on obtient

$$\phi A_1=\tfrac{1}{2}\,|\,b-c\,|\sqrt{\frac{R}{R-2r}},$$

* *Mathematical Gazette*, XVIII (October 1934), p. 271.

et, par analogie,

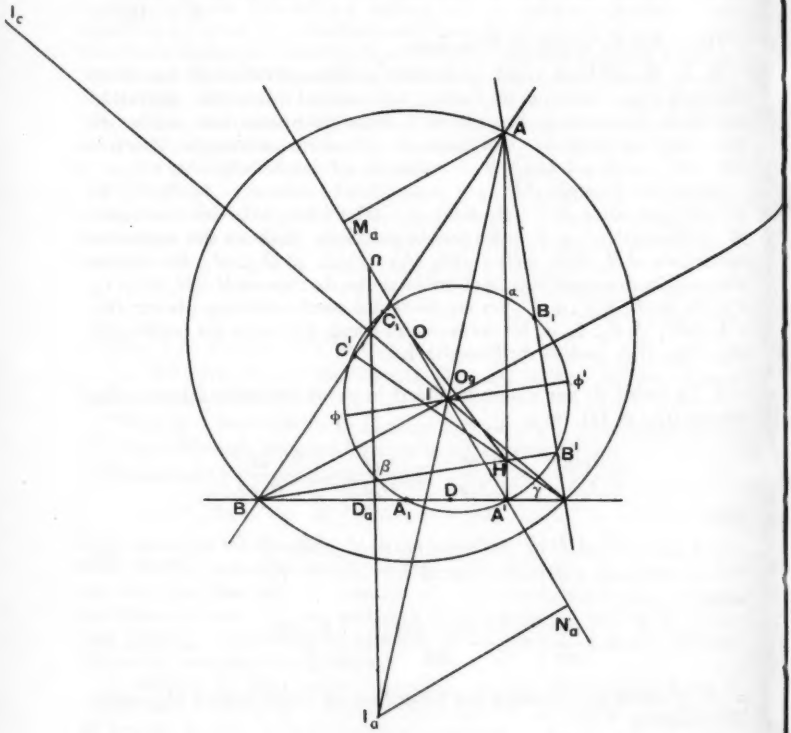
$$\phi B_1=\tfrac{1}{2}\,|\,c-a\,|\,\sqrt{\frac{R}{R-2r}},\quad \phi C_1=\tfrac{1}{2}\,|\,a-b\,|\,\sqrt{\frac{R}{R-2r}},^{*}\quad \ldots(4)$$

puis,

$$\phi A'=\frac{(b-c)(p-a)}{a}\sqrt{\frac{R}{R-2r}},$$

et, par analogie,

$$\phi B'=\frac{(c-a)(p-b)}{b}\sqrt{\frac{R}{R-2r}},\quad \phi C'=\frac{(a-b)(p-c)}{c}\sqrt{\frac{R}{R-2r}},^{*}\ \ldots(5)$$



enfin

$$\phi\alpha=\tfrac{1}{2}\,|\,r+r_a-2R\,|\,\sqrt{\frac{R}{R-2r}},$$

* Nous avons obtenu ces expressions d'une autre manière (*Annales de la Société scientifique de Bruxelles*, 1932, p. 289). (V. T.)

et, par analogie,

$$\phi\beta=\tfrac{1}{2}\,|\,r+r_b-2R\,|\,\sqrt{\frac{R}{R-2r}},\quad \phi\gamma=\tfrac{1}{2}\,|\,r+r_c-2R\,|\,\sqrt{\frac{R}{R-2r}}. \qquad \text{.....(6)}$$

De ces relations on déduit, en supposant, pour fixer les idées, que $b<a<c$,

$$\phi B_1+\phi C_1=\phi A_1,\quad b\,.\,\phi B'+c\,.\,\phi C'=a\,.\,\phi A',*$$
$$a\,.\,\phi\bar{\alpha}+b\,.\,\phi\bar{\beta}+c\,.\,\phi\bar{\gamma}\equiv 0. \qquad \text{.....(7)}$$

D'où ce théorème : *La distance du point ϕ de Feuerbach au milieu du côté moyen d'un triangle ABC égale la somme de ses distances aux milieux des deux autres côtés.*†

N.B.—Les formules (5) conduisent aisément à l'identité

$$(b-c)(p-a)\cos A+(c-a)(p-b)\cos B+(a-b)(p-c)\cos C\equiv 0.$$

2. Soient M_a, M_b, M_c et N_a, N_b, N_c les projections orthogonales des points A, B, C et I_a, I_b, I_c sur l'axe IO. On a, par exemple,

$$\frac{N_aI_a}{AM_a}=\frac{II_a}{AI}=\frac{D_aD}{DA'}=\left|\frac{a}{p-a}\right|.$$

Or, les expressions (4) et (5) donnent

$$\frac{\phi A_1}{\phi A'}=\frac{a}{2(p-a)}=\frac{A_1D}{DA'}=\frac{1}{2}\left|\frac{D_aD}{DA'}\right|=\frac{1}{2}\left|\frac{N_aI_a}{AM_a}\right|.$$

De plus, nous avons montré que *les distances du point ϕ de Feuerbach aux pieds des hauteurs sont égales à celles des sommets du triangle à l'axe IO.*‡

On a donc, puis, par analogie,

$$\phi A_1=\tfrac{1}{2}N_aI_a,\quad \phi B_1=\tfrac{1}{2}N_bI_b,\quad \phi C_1=\tfrac{1}{2}N_cI_c. \qquad \text{.....(8)}$$

D'où cette propriété que nous croyons nouvelle : *Les distances du point ϕ de Feuerbach aux milieux des côtés d'un triangle ABC sont égales à la moitié de celles des centres des cercles exinscrits à la droite IO qui joint les centres des cercles inscrit et circonscrit au triangle.*

N.B.—Nous avons prouvé également que les longueurs des tangentes du point ϕ aux cercles exinscrits (I_a), (I_b), (I_c) sont respectivement égales à $2\phi A_1$, $2\phi B_1$, $2\phi C_1$.§ Nous pouvons donc ajouter que :

Les distances du point ϕ de Feuerbach d'un triangle aux contacts des tangentes tracées de ce point ϕ aux trois cercles exinscrits, sont égales aux distances des centres I_a, I_b, I_c de ces cercles à la droite IO qui unit les centres des cercles inscrit et circonscrit au triangle.

3. Soit ϕ' le point diamétralement opposé au point ϕ de Feuerbach

* V. Thébault, *Mathesis*, 1912, p. 190.

† W. Godt, *Journal de Grunert*, 1886, p. 436.

‡ *Nouvelles Annales de Mathématiques*, 1910, p. 274.

§ *Annales de la Société scientifique de Bruxelles*, 1932, p. 292.

sur le cercle des neuf points. Le rayon I_aD_a du cercle exinscrit (I_a) rencontre la droite IO au symétrique Ω du point I par rapport au centre O, et le point Ω est le centre du cercle circonscrit, de rayon $2R$, au triangle $I_aI_bI_c$. En vertu des égalités (8), et de ce que $\phi\phi'=R=\frac{1}{2}I_a\Omega$, les triangles $\phi\phi'A_1$, $I_a\Omega N_a$ sont semblables, et on a

$$\angle\phi'\phi A_1=\angle\Omega I_aN_a=\angle(BC, IO).$$

Dès lors, *les angles des droites, ϕA_1, ϕB_1, ϕC_1 avec la tangente en ϕ aux cercles inscrit et des neuf points sont égaux aux compléments des angles formés par l'axe IO avec les côtés du triangle ABC.*

Remarque. Pour fixer les idées, nous avons considéré dans tout ce qui précède le point ϕ de Feuerbach relatif au cercle (I) inscrit au triangle fondamental. Les propriétés obtenues s'appliquent sans difficulté aux trois autres points ϕ_a, ϕ_b, ϕ_c. V. THÉBAULT.

1193. *The elementary theorems in analytical statics.*

The sum of the moments of forces P_1, P_2, ... acting along the lines $p_1-x\cos\alpha_1-y\sin\alpha_1=0$, etc., about (h, k) is

$$\Sigma P_1(p_1-h\cos\alpha_1-k\sin\alpha_1),$$

or

$$\Sigma P_1p_1-h\Sigma P_1\cos\alpha_1-k\Sigma P_1\sin\alpha_1,$$

which is usually written

$$G+kX-hY. \qquad \text{.....(i)}$$

Thus the moment of the system about any point is known provided that X, Y and G are known. We say that the system has coordinates X, Y, G, and refer to it as the system (X, Y, G).

A couple is a system of coplanar forces having (a) constant moment, (b) zero component in all directions. The necessary and sufficient conditions for (X, Y, G) to be a couple are easily proved to be $X=Y=0$, $G\neq0$, so that $(0, 0, G)$ is a couple.

The expression (i) can be written

$$\sqrt{(X^2+Y^2)}\left\{\frac{G+kX-hY}{\sqrt{(X^2+Y^2)}}\right\},$$

unless $X=Y=0$, and is the moment of a single force $\sqrt{(X^2+Y^2)}$, acting along $G+yX-xY=0$.

So *a system of coplanar forces can be reduced to a single force or a couple.*

By consideration of (i) it appears that the system (X_1, Y_1, G_1) plus the system (X_2, Y_2, G_2) has the same moment about all points as the system $(X_1+X_2, Y_1+Y_2, G_1+G_2)$.

Hence the addition theorem

$$(X_1, Y_1, G_1)+(X_2, Y_2, G_2)=(X_1+X_2, Y_1+Y_2, G_1+G_2).$$

Consequently, since $(0, 0, G)+(0, 0, -G)=(0, 0, 0)$, *two couples of equal and opposite moment are in equilibrium*, and since

$$\Sigma(0, 0, G)=(0, 0, \Sigma G),$$

the resultant of a number of coplanar couples is a couple of moment equal to the sum of the moments of the couples.

Moreover, since

$$(X, Y, G_1)+(0, 0, G_2)=(X, Y, G_1+G_2),$$

the resultant of a force and a couple is a force parallel to the first force (the direction in both cases making an angle arc tan Y/X with the axis of x).

Again,

$$(X, Y, G)=(X, Y, 0)+(0, 0, G).$$

Therefore *a system of coplanar forces may be reduced to a force acting at a given point together with a couple* (the origin being taken at the given point). A. C. HEATH.

1194. *Note on Spherical Trigonometry.*

1. It is perhaps worth while to adopt a symmetric method of procedure and proof and give a synoptic view of what Felix Klein calls the formulae of the first kind in spherical trigonometry, which connect the cosines and sines of the sides and angles of a spherical triangle.

Let ABC be a spherical triangle and O the centre of the sphere. Take any point P on OA and draw perpendiculars PD, PE, PF on OB, OC and the plane BOC to meet them in D, E, F respectively. Join DF, EF, OF.

From elementary solid geometry, it is easily seen that FD is perpendicular to OB, FE to OC, $P\hat{D}F=B$, $P\hat{E}F=C$; and the space-configuration of five points $(OPDEF)$ has imbedded in it five elements (a, b, c, B, C) of the triangle ABC. It must therefore be possible to prove all the relations connecting *four* of these elements from this single figure.

This we now proceed to do by projecting OE, EF and OF on OD and FD in succession.

Evidently, $$OD=OE\cos a+EF\sin a,$$

so that
$$\cos c=\frac{OD}{OP}=\frac{OE}{OP}\cos a+\frac{EF}{EP}\cdot\frac{EP}{OP}\sin a$$
$$=\cos b\cos a+\cos C\sin b\sin a. \quad \ldots\ldots\ldots(1)$$

Again,
$$\frac{\sin b}{\sin B}=\frac{PE}{OP}\Big/\frac{PF}{PD}=\frac{PD}{OP}\Big/\frac{PF}{PE}=\frac{\sin c}{\sin C}. \quad \ldots\ldots\ldots(2)$$

Further, $FD=OE\sin a-EF\cos a$, so that

$$\frac{FD}{PD}\cdot\frac{PD}{OP}=\frac{OE}{OP}\sin a-\frac{EF}{EP}\cdot\frac{EP}{OP}\cos a,$$

i.e. $$\cos B\sin c=\cos b\sin a-\cos C\sin b\cos a. \quad \ldots\ldots(3)$$

Dividing by $\sin b$ and using (2), we transform (3) into

$$\cot B\sin C=\cot b\sin a-\cos C\cos a. \quad \ldots\ldots\ldots(4)$$

2. If the triangle ABC be right-angled at B, the points F and D coincide in our construction and we easily prove that

$$\cos b = \cos c \cos a, \quad \text{.....(5)}$$

and

$$\cos C = \tan a \cot b. \quad \text{.....(6)}$$

We now enunciate the following "*principle of substitution*": *Any relation among the sides and angles of a spherical right-angled triangle ABC with $B=\frac{1}{2}\pi$, remains true when for the elements (a, b, c, C) are substituted respectively $(\frac{1}{2}\pi - C, \frac{1}{2}\pi - c, \frac{1}{2}\pi - b, \frac{1}{2}\pi - a)$, A and B remaining unchanged.*

This is because, for every spherical triangle ABC in which $B=\frac{1}{2}\pi$, there exists another triangle $A'B'C'$, such that $a'=\frac{1}{2}\pi - C$, $b'=\frac{1}{2}\pi - c$, $c'=\frac{1}{2}\pi - b$, $A'=A$, $B'=B$, $C'=\frac{1}{2}\pi - a$, as is easily verified by producing CA to B' and BA to C' so that $CB'=BC'=\frac{1}{2}\pi$, and comparing the elements of the two triangles ABC and $AB'C'$. We have only to remember that C' is the pole of BC and C is the pole of $B'C'$.

Using the above principle, we derive from (5) and (6)

$$\sin c = \sin b \sin C, \quad \text{.....(7)}$$

and

$$\sin a = \cot C \tan c. \quad \text{.....(8)}$$

Applying the same principle to the analogues of (7) and (8), viz.:

$$\sin a = \sin b \sin A \quad \text{and} \quad \sin c = \cot A \tan a,$$

we have,

$$\cos C = \cos c \sin A, \quad \text{.....(9)}$$

and

$$\cos b = \cot A \cot C. \quad \text{.....(10)}$$

Thus, we have established the fundamental formulae in spherical trigonometry as simply as possible.

3. We wish finally to remark that, if the formulae (5), (7), (9) be made fundamental, the formulae (1), (2), (3) can be easily deduced from them by drawing AD perpendicular to BC.

For, from the right-angled triangles CDA, BDA,

$$\cos C = \cos AD \sin C\hat{A}D = \cos AD \sin CD/\sin b, \quad \text{.....(11)}$$

$$\cos b = \cos AD \cos CD, \quad \text{.....(12)}$$

$$\cos c = \cos AD \cos BD, \quad \text{.....(13)}$$

$$\cos B = \cos AD \sin BD/\sin c. \quad \text{.....(14)}$$

From (11), (12), (13) we derive (1), and from (12), (13), (14) we get (3), using the expressions for $\cos BD$, $\sin BD$ in terms of sines and cosines of BC, CD. The proof of (2) is too simple to deserve an explicit mention.

A. A. K. AYYANGAR.

1195. *The reduction of the mass-accelerations and the momentum of a rigid lamina.*

A note by Professor H. V. Lowry in the July *Gazette*, pp. 211-213, on the use of the instantaneous centre, suggested to us that it would be interesting to make clear what points, A, of the rigid lamina, could be used in obtaining the moment equation there referred to as $L_A = I_A\dot{\omega}$.

We shall use the notation of the latter part of the article referred to. Let A be a point (x_1, y_1) of a rigid lamina, relative to axes fixed in space. Let any point of the lamina have coordinates (x, y), or (x_2, y_2) relative to A, so that

$$x = x_1 + x_2 \quad \text{and} \quad y = y_1 + y_2.$$

Now the mass-accelerations of the system of particles of the body form a system of localised vectors and hence, like forces, can be reduced to (*a*) a single mass-acceleration acting at A, and (*b*) a mass-acceleration couple.

(*a*) The single mass-acceleration

$$= \Sigma(m\ddot{x}, m\ddot{y}),$$

$m\ddot{x}$ and $m\ddot{y}$ being the two components of the mass-acceleration of any particle m of the lamina,

$$= \frac{d^2}{dt^2} \Sigma(mx, my)$$

$$= \frac{d^2}{dt^2} (M\bar{x}, M\bar{y}),$$

where $(\bar{x}, \bar{y})$ is the centre of gravity of the body,

$$= (M\ddot{\bar{x}}, M\ddot{\bar{y}}),$$

which is the mass-acceleration of a particle of mass M situated at the centre of gravity of the system, G, and moving with the acceleration of G.

(*b*) The mass-acceleration couple is the sum of the moments of the mass-accelerations of the particles about A

$$\begin{aligned} &= \Sigma m(x_2\ddot{y} - y_2\ddot{x}) \\ &= \Sigma m\{x_2(\ddot{y}_1 + \ddot{y}_2) - y_2(\ddot{x}_1 + \ddot{x}_2)\} \\ &= \ddot{y}_1\Sigma(mx_2) - \ddot{x}_1\Sigma(my_2) + \Sigma m(x_2\ddot{y}_2 - y_2\ddot{x}_2) \\ &= M(\bar{x}_2\ddot{y}_1 - \bar{y}_2\ddot{x}_1) + I_A\dot{\omega}, \end{aligned}$$

where $(\bar{x}_2, \bar{y}_2)$ is the centre of gravity relative to A.

This reduces to $I_A\dot{\omega}$ in three cases,

(i) A is the centre of gravity, since then $\bar{x}_2 = \bar{y}_2 = 0$;

(ii) A is moving with a constant velocity or is a fixed point, since $\ddot{x}_1 = \ddot{y}_1 = 0$, which includes the special case of rotation about a fixed axis; or

(iii) the acceleration of A is parallel to AG, since then $\frac{\ddot{y}_1}{\ddot{x}_1} = \frac{\bar{y}_2}{\bar{x}_2}$. As shown in the note referred to, this sometimes includes the case when A is at the instantaneous centre.

The equations of motion can now be obtained by expressing the equivalence of the forces and the reduced mass-accelerations; in particular, $L_A = I_A\dot{\omega}$ follows by taking moments about A.

Momentum can be treated in a similar way. For the momenta of the particles again form a system of localised vectors which can be

reduced to (*a*) a single momentum at A, the linear momentum of the lamina, and (*b*) a momentum-couple called the angular momentum of the lamina about A.

(*a*) The linear momentum

$$= \Sigma(m\dot{x}, m\dot{y})$$
$$= (M\dot{\bar{x}}, M\dot{\bar{y}}).$$

(*b*) The angular momentum about A

$$= \Sigma m(x_2\dot{y} - y_2\dot{x})$$
$$= \Sigma m\{x_2(\dot{y}_1+\dot{y}_2) - y_2(\dot{x}_1+\dot{x}_2)\}$$
$$= M\bar{x}_2\dot{y}_1 - M\bar{y}_2\dot{x}_1 + I_A\omega.$$

This reduces to $I_A\omega$ in the three cases:

(i) A is the centre of gravity, since then $\bar{x}_2=\bar{y}_2=0$;

(ii) the velocity of A is zero, since then $\dot{x}_1=\dot{y}_1=0$. Hence A is a fixed point or is at the instantaneous centre.

(iii) The velocity of A is parallel to AG, since then $\dfrac{\dot{y}_1}{\dot{x}_1}=\dfrac{\bar{y}_2}{\bar{x}_2}$.

Momentum equations can now be obtained by expressing the equivalence of the impulses and the change of momentum of the lamina.

This treatment is developed fully in Palmer and Snell's *Mechanics*.

K. S. Snell and A. H. G. Palmer.

1196. *A Lewis Carroll Problem.*

Referring to the Lewis Carroll Problem, mentioned in the *Gazettes* of May 1933 and May 1935, namely, to find three equal rational sided right-angled triangles, Diophantus in his *Arithmetica* referred to the triangle whose sides are $2mn$, m^2-n^2, m^2+n^2 as being formed from the numbers m, n. In Book V he set out to find three rational right-angled triangles of equal area and tacitly assumes (at least, he gave no proof) that if $ab+a^2+b^2=c^2$, the triangles formed from (c, a), (c, b) and $(c, a+b)$ have equal areas. It is easily shown that in each case the area is $abc(a+b)$.

We can find a general formula by writing the assumption in the form

$$a(a+b)=c^2-b^2=(c-b)(c+b).$$

Then put
$$ma=n(c+b),$$
$$n(a+b)=m(c-b),$$

and it follows easily that

$$a : b : c = 2mn+n^2 : m^2-n^2 : m^2+mn+n^2,$$

so that we can take for a, b, c numbers proportional respectively to $2mn+n^2$, m^2-n^2, m^2+mn+n^2.

Putting $m=2$, $n=1$, we have $a=5$, $b=3$, $c=7$ and obtain the triangles (42, 40, 58), (70, 24, 74), (112, 15, 113).

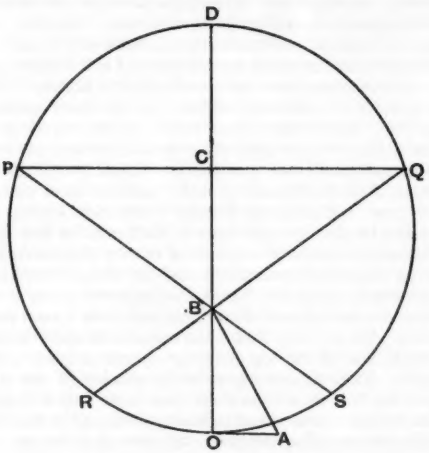
This is the set obtained by Diophantus himself.

Putting $m=3$, $n=1$, we find $a=7$, $b=8$, $c=13$ and the triangles

are (120, 182, 218), (105, 208, 233) and (56, 390, 394), which is the second set given by Mr. Todhunter in the *Gazette*, May 1935. Of course, numerous other sets may be obtained ; mentioning only one, let us put $m=3$, $n=2$, then $a=16$, $b=5$, $c=19$ and the triangles are (105, 608, 617), (336, 190, 386), (80, 798, 802). J. P. McCarthy.

1197. *Construction of a regular pentagon.*

Draw a right angle AOD. From OD cut off OB equal to $2OA$, and from BD cut off BC, CD each equal to BA. Describe a circle



on OD as diameter and draw PCQ a chord perpendicular to OD. Join QB and PB and produce them to meet the circumference in S and R.

Then $DPRSQ$ is a regular pentagon. F. C. Boon.

1057. I have but one book with me and that is Euclid, but I begin to be tired of him. I believe he has done more harm than good. He has set fools a-reasoning.—Hobbes ; letter written by St. Evremond to Waller. [Per Mr. A. Buxton.]

1058. The earliest of the exact sciences was geometry. Some of the elder amongst us had the advantage of beginning our studies in it under the guidance of Euclid. I confess I look back to Euclid with unfeigned delight. He teaches us wonderful lessons in accurate thinking in our early years, and he stirs very deep thoughts when we come to reflect upon his methods after decades of varied thought and experience have passed over our heads.—Archbishop D'Arcy, *Providence and the World Order*.

1059. I remember the admiration and delight with which, on the basis of Newton's abstractions, I followed the demonstration that bodies in space and time and moving under the law of inverse squares must always follow paths which are conic sections.—Archbishop D'Arcy, *Providence and the World Order*.

REVIEWS.

An Examination of Examinations. By Sir Philip Hartog and E. C. Rhodes. Pp. 81. 1s. 1935. (Macmillan)

This report of the International Institute Examinations Enquiry is for the most part a summary only; the detailed statistical analysis upon which the present report is based is promised the reader in a second and larger volume, *The Marks of Examiners*.

Sir Philip Hartog's committee obtained from various examining bodies, "after long and delicate negotiations," some scripts actually worked in the course of ordinary examinations. They then arranged for these scripts to be re-marked by examiners chosen by them, who were "in every case examiners with experience of the kind of examination investigated" and who "were all paid in accordance with the usual scale adopted for the marking of scripts of the same kind or, in certain cases, on a scale slightly higher." The committee regarded payment as an essential condition of the investigation, because of its effect upon the "psychological condition" of the examiner. As a result, very substantial differences of standard between the examiners were disclosed.

The scripts investigated were drawn from School Certificate examinations in History, Latin, French, Chemistry and English; from two Special Place examinations; from a University English Essay Scholarship examination; and from Honours Degree examinations in Mathematics and History.

In the mathematical examination each of twenty-three scripts was marked independently by each of six examiners and the scripts were then revised by the examiners working in pairs. There was, apparently, no consultation between the examiners, no scheme of marking, not even agreed maxima for the various questions. We are told that "the examiners agree in their placing of the first two candidates at the top of the group and in placing the thirteenth in order of merit. They do not agree in the placing of the other twenty." Those readers of the *Gazette* who in their time took Tripos examinations will doubtless notice that even this result indicates *some* skill in diagnosis, the odds against even this amount of agreement being arrived at by pure chance being millions to one. The report, unfortunately, shows no sign that it is aware that it is paying even a limited compliment to its examiners.

The committee "are anxious that their investigations should not be interpreted as a criticism of any particular body"—or indeed, one hopes, of examining bodies as a whole. Nor do they wish to imply that examinations are so unreliable that they should be abolished; they desire to keep them, and to retain, moreover, the essay type of question which gives rise to the greatest difficulties of assessment. But they cannot be entirely absolved from responsibility if the general public, reading such phrases as "it is only by careful and systematic experiment that methods of examination can be devised not liable to the distressing uncertainties of the present system", leaps to the conclusion that no careful and systematic experiments have yet been made, and that the report is an attack upon the examining system of the country. The report has been reviewed widely by the general press, not the educational press alone, and its summarised conclusions have disturbed many intelligent members of the general public who do not know that no existing examining body would be content to carry out its work in the haphazard and casual way followed in this investigation by, for example, the examiners in School Certificate History. These gentlemen certainly disclosed very wide discrepancies; moreover, when the fourteen examiners each re-marked the fifteen scripts after an interval of a year, they changed their minds as to the verdict of failure, pass or credit in 92 cases out of the 210. But apparently there was no examiners' meeting,

there was not even a common marking scheme, and the number of scripts was insufficient for any examiner to " get a standard ". Besides making provision for these obvious points, an examining body would have had trial scripts marked, specimens of the marking of every examiner revised by a chief examiner, and a final revision of border-line cases made. Sir Philip Hartog's examiners worked under conditions not even remotely resembling the conditions which obtain in School Certificate examinations.

Another feature of this historical investigation should be noted. The scripts examined were all scripts to which the examining body had previously allocated the *same* " middling " mark. Two unfortunate results follow. First, the variations disclosed were not typical of the treatment of candidates as a whole, as many reviewers have assumed them to be, but only of the notoriously difficult border-line patch which in practice is never left to the uncoordinated decisions of individual examiners. It is a reasonable inference that nothing approaching 92 decisions out of 210 would have been altered had the scripts constituted a fair sample. And second, this method of selection placed the examiners at a disadvantage. They had neither very good nor very bad scripts with which to make comparisons and so get their standards. They would expect something approaching a normal distribution of marks ; and Sir Philip Hartog took care that they should not get it. It is curious that a committee so anxious to make the conditions of marking " those of a real examination ", and so concerned about the " psychological condition " of their examiners, should have overlooked a point of this kind.

It is fair to add that in most of the other investigations many of the points mentioned above received attention ; but it is fair also to report that the discrepancies of marking then revealed, though often considerable, were not of the order disclosed by the historical investigation.

The reviewer, who has some experience of School Certificate examining in Mathematics, was curious to see how the committee dealt with that branch. They ignored it entirely. He is tempted, therefore, to give his own opinion. In a School Certificate examination in Mathematics, in which the usual precautions are taken, the average script is rarely altered more than 1 or 2 per cent. upon revision. Larger variations, as a rule, imply a blunder ; and the type of examiner who blunders frequently is detected, and removed, at an early stage of his career. In other words, it is usually possible to secure *uniformity* of marking between different examiners. This does not solve the harder problem of *validity* of marking ; all that can be said about that is that, in practice, the order of merit submitted by the schools does correspond reasonably well with the order as returned by the examiners. An exact correspondence cannot be expected, if only for the reason that no one can guarantee that a candidate's work under examination conditions represents his real standard. For a number of reasons, some of them probably removable, others uncontrollable either by the examiner, the teacher or the candidate, a candidate not infrequently fails to do himself justice.

There are two aspects of mathematical examining at this stage which seem to the reviewer to be more worth investigation than the aspects dealt with in this report. Probably neither lends itself to statistical analysis, which may be the reason why Sir Philip Hartog and Dr. Rhodes ignore both. The first is connected with the setting of examination papers : how are we to set questions which do not give a premium to the " academically minded " candidate—who should *not* be our concern at this stage—and which are at the same time not so completely straightforward as to encourage unintelligent cramming ? The second concerns the effect of examinations upon teaching. An examiner, dealing with ten or twenty thousand candidates, can take no undue risks with

unconventional questions; also, he must set questions of a type which are assessable by a large panel of colleagues. These conditions have a restrictive effect upon him, and there is a resulting risk, if the schools follow the examination too closely, that preparation may be directed mainly if not exclusively to a type of work not completely representative of what mathematical work in schools should be. Opinions differ as to the extent of this warping effect which external examinations tend to have upon schools, but it certainly exists in some measure; and examiners would be as glad as teachers if someone could show them how it could be avoided. B. A. H.

Intrinsic Geometry of Ideal Space. I., II. By A. R. FORSYTH. Pp. xxvi, 553; xiv, 655. £6 6s. 1935. (Macmillan)

This book is concerned with sub-spaces of a euclidean space of an arbitrary number of dimensions. The differential geometry of such a sub-space is developed in great detail, the greater part of the book being devoted to curved surfaces and three-dimensional loci. It is curious that the title of the book leads one to expect the antithesis of what the subject matter actually is. For the term "intrinsic geometry" is generally, and appropriately, used to mean the study of a space without reference to any space of larger dimensionality in which it may be imbedded.

I think the most interesting passages are those which deal with geodesic triangles and parallelograms and their relation to Gaussian and Riemannian curvature. For example, a sequence of typical sections (§§ 109-114) contains an account of a small geodesic triangle, of which two sides and the included angle are given. The length of the third side and the two remaining angles are calculated up to, and including, the third order terms. Section 114 contains the well-known theorem relating the difference between π and the sum of the angles to the Gaussian curvature of the area enclosed by the triangle. Similar calculations are applied to the study of geodesic parallelograms, the term "parallelogram" being interpreted in various ways. These passages are welcome, as the study of such simple geodesic figures is, perhaps, the best way of getting an insight into the meaning of Riemannian curvature, and so into the structure of a space with a Riemannian metric.

But, taken as a whole, the book is much too long. It is loaded with results of no general interest, which appear as the product of laborious calculations. Also the book suffers from the fact that, with one or two exceptions, such as Levi-Civita's theory of parallel displacement, the advances made during the last twenty years have been ignored. This independence on the part of the author has led to a startling error. For it has been shown by E. Cartan (*Annales de la Soc. Polonaise de Math.*, Vol. VI (1927), pp. 1-7) that a fragment, at least, of any n-dimensional space carrying a given (positive definite) Riemannian metric can be imbedded in a euclidean space of $n(n+1)/2$ dimensions. For example, if E, F and G are given functions of p and q, with continuous partial derivatives up to and including the third order, subject to the conditions $E>0$, $EG-F^2>0$ but otherwise arbitrary, there is a surface in three-dimensional space given by

$$x=x(p, q),\ y=y(p, q),\ z=z(p, q) \tag{1}$$

such that

$$dx^2+dy^2+dz^2=E\,dp^2+2F\,dp\,dq+G\,dq^2.$$

However, on p. 23 of the first volume the author states that the contrary is the case, and in § 89 he attempts to prove that a functional relation between E, F, G and their derivatives must be satisfied in order that such a surface may exist.

The argument put forward by the author may be summarised as follows. Given a quadratic differential form

$$ds^2 = E\,dp^2 + 2F\,dp\,dq + G\,dq^2,$$

we may assume without loss of generality that the parametric curves are mutually orthogonal—that is to say, $F=0$. If ds^2 is the line element of a surface in ordinary space let us further assume that the parametric curves are the lines of curvature. That is to say, $M=0$, where L, M and N are the coefficients of the second fundamental form. Then

$$LN = KEG,$$

where K is the Gaussian curvature and depends only on E, G and their first and second order derivatives. Substituting KEG/L for N in the Mainardi-Codazzi equations, we are led to two partial differential equations of the form

$$\frac{\partial L}{\partial p} = R(p, q, L), \quad \frac{\partial L}{\partial q} = S(p, q, L),$$

where the functions R and S depend only on L, E, G and the derivatives of E and G. The integrability condition can be solved to give L in terms of E, G and their derivatives, and so each of the two differential equations provides a relation between E, G and their derivatives (up to and including derivatives of the fifth order). Moreover, these two relations, which we shall call R_1 and R_2, are seen to be independent, in the sense that they are expressed in terms of independent functions of E, G and their derivatives.

However, we have not only required that the line element ds^2 shall be imbedded in euclidean space, but have further required two mutually orthogonal systems of curves, chosen at random, to be the lines of curvature. This implies a condition, namely $M=0$.

I now quote from p. 235 (Vol. I), where the argument breaks down. "There are two equations in virtue of which it (the condition $M=0$) can be satisfied; after it has been satisfied, there survives one further relation, independent of that condition." Actually the relations R_1 and R_2 have been derived as a consequence of the condition $M=0$, which is equivalent to $LN = K(EG - F^2)$ (here $F=0$). A priori it would be very surprising if either of these relations were, as the author asserts, independent of the condition $M=0$. Since the theorem which the author sets out to disprove is in fact true we know that neither relation is independent of this condition. We also know that the parameters (p, q) can be transformed to parameters (p', q') such that the functions E', G' and their derivatives are related in the way indicated by the author, where

$$E' = E\left(\frac{\partial p}{\partial p'}\right)^2 + G\left(\frac{\partial q}{\partial q'}\right)^2, \quad G' = E\left(\frac{\partial p}{\partial p'}\right)^2 + G\left(\frac{\partial q}{\partial q'}\right)^2,$$

$$F' = E\frac{\partial p}{\partial p'}\frac{\partial p}{\partial q'} + G\frac{\partial q}{\partial p'}\frac{\partial q}{\partial q'} = 0.$$

This discussion refers only to the existence of a fragment of a surface, given by equations of the form (1) with suitable restrictions imposed upon p and q (*e.g.*, $-1<p, q<1$). The questions relating to the possibility of imbedding a complete Riemannian space in euclidean space are far more difficult to answer. For example, it has recently been shown by L. Bieberbach (*Comm. Math. Helvetici*, Vol. 4 (1932), pp. 248-55) that the hyperbolic plane can be imbedded in Hilbert space, which has infinitely many dimensions. But whether or no it can be imbedded, in its entirety and without singularities, in euclidean space of any finite dimensionality is still an open question. J. H. C. W.

Interpolation and Approximation by Rational Functions in the Complex Domain. By J. L. WALSH. Pp. ix, 382. $5.00. 1935. American Mathematical Society Colloquium Publications, 20. (American Mathematical Society)

The title of this work indicates a field too wide to be dealt with conveniently in a single treatise. Professor Walsh has taken up what might be called the Runge-Faber-Bernstein standpoint and has treated the subject exhaustively from this point of view. The present formulation is largely his own work—he has made some forty contributions in the last twelve years—but many of the principles are much older. As the subject is rather a technical one it may be well to give a rough sketch of its roots and connections.

Suppose that $f(z)$ is regular in a closed region C bounded by a contour Γ, so that

$$f(z)=\frac{1}{2\pi i}\int_\Gamma \frac{f(t)}{t-z}\,dt \quad (z \text{ in } C).$$

Now we can approximate to the integral on the right by the Riemann sum

$$\frac{1}{2\pi i}\sum_{k=1}^{n}\frac{f(t_k)}{t_k-z}(t_{k+1}-t_k) \quad (t_1, t_2, \dots \text{ points on } \Gamma),$$

which is a rational function. More generally, there may be several disconnected closed regions $C_1, \dots, C_n$ and distinct functions $f_1(z), \dots, f_n(z)$. A rational function $r(z)$ can be found such that $|f_1(z)-r(z)|<\epsilon$ in C_1, $|f_2(z)-r(z)|<\epsilon$ in C_2, etc. Moreover, the poles of $r(z)$ can be assigned arbitrarily, provided that there is at least one in each connected domain forming part of the complement of $C_1+C_2+\dots+C_n$. This is Runge's theorem (1885). In particular, if this complement is a single connected domain including the point at infinity, the pole can be taken to be this point, *i.e.*, $r(z)$ can be chosen to be a polynomial.

Another idea now makes its appearance. In the case just mentioned we have proved the existence of a polynomial $p(z)$ such that

$$(1) \qquad |f(z)-p(z)|<\epsilon \quad (z \text{ in } C).$$

What can be said about the degree of this polynomial? It appears that there is a sequence $p_0(z), p_1(z), \dots$ of degrees 0, 1, ... such that

$$|f(z)-p_n(z)|<Mr^n \quad (r<1).$$

Such a sequence is said to *converge maximally* to $f(z)$. The property specified in (1) is called *approximation in Tchebycheff's sense*. Instead of it we might demand that

$$(2) \qquad \int_\Gamma w(z)\,|f(z)-p_n(z)|^k\,|dz| \quad \text{or} \quad \iint_C w(z)\,|f(z)-p_n(z)|^k\,ds$$

should be as small as possible for given n, $w(z)$ being some assigned *weight function*. Sequences of polynomials approximating to $f(z)$ in various senses can be defined in this way, and it appears that they also converge maximally to $f(z)$. The integrals (2) are obviously connected with the Riesz-Fischer theorem and sure enough polynomials *orthogonal on a curve* Γ, *i.e.* such that

$$\int_\Gamma p_k(z)\overline{p_l(z)}\,|dz|=0 \quad (k\neq l),$$

are the subject of a brilliant memoir of Szëgo.

We have no space to go into the matter further. Speaking generally, the subject may be said to belong to the industrial area of function theory, though

there are occasional beauty spots. Too much depends on the detailed conditions. For example, Faber's problem of finding a set of polynomials belonging to a point set C, *i.e.* such that any function analytic on C can be expanded in a series

$$f(z)=a_0p_0(z)+a_1p_1(z)+\ldots,$$

is solved for point sets of seven different types. The reader will find a complete account of this and much more of importance like it, but it needs a mathematical Gargantua to digest it. J. M. W.

Theorie der Orthogonalreihen. By S. KACZMARZ and H. STEINHAUS. Pp. vi, 298. $5. 1935. Monografje Matematyczne, 6. (Warsaw)

This book gives a good account of recent researches in orthogonal series, and may be recommended not only to specialists in that subject but also to those interested in trigonometric series and in linear operations. The book is interesting throughout. The authors have done much to simplify the proofs; the most striking characteristic of the book is the effective, indeed the triumphant, use of the theory of linear operations.

The first chapter contains some preliminary theorems (usually with proofs) on series, integrals and linear operations. A great part of this chapter might well have been placed half-way through the book. The second chapter explains the fundamental concepts of the subject. The attractive third chapter is devoted to orthogonal developments in L^2. There is an account of Schmidt's method of orthogonalisation; of the expression for the best approximation in L^2 by means of Gram's determinants; and of Müntz's generalisation of Weierstrass' approximation theorem.

In the fourth chapter, a number of special systems of orthogonal functions are considered. On the one hand, the systems of Haar, Rademacher and Walsh; on the other hand, the polynomials of Legendre, Tchebycheff, Laguerre and Hermite. Concerning the systems of the second class, as only the most elementary properties are considered, I think that they might well have been omitted. The space thus saved could have been devoted to giving fuller proofs throughout the rest of the book.

The fifth chapter is concerned with convergence and summability almost everywhere. The next chapter contains an account of orthogonal developments in the spaces $L^p(p \geqslant 1)$, and the spaces of bounded and of continuous functions. Particularly noteworthy is the treatment of "multipliers", that is, sequences (λ_n) which have the property of transforming the orthogonal development $\Sigma a_n\phi_n$ of any function of an assigned class, into the orthogonal development $\Sigma\lambda_n a_n\phi_n$ of a function of an assigned class. Proofs of the theorems of F. Riesz and of Paley are given. Strangely enough the authors, who are usually fond of appealing to the properties of linear operations, do not appeal to the convexity theorem of M. Riesz, which furnishes the simplest proof of the theorems in question. The theorems involve a parameter p, which is restricted in each theorem by $1<p<2$. The authors consider orthogonal systems for which these theorems are no longer true when $p>2$.

The seventh chapter contains an account of lacunary series. The final chapter is devoted to bi-orthogonal systems, and to systems of relatively orthogonal polynomials.

The book contains many misprints. There are some obscurities in the enunciations and in the proofs; for example, the definition of the functions $f_{p,m}(t)$ at the foot of p. 166, with the explanation on p. 167, is incomprehensible to me. I am somewhat perplexed by the unequal style of the book. In the first chapter, more than a page is devoted to Weierstrass' approximation

theorem; this is surely familiar to everyone likely to read the book. On p. 39, the authors define a set of functions, which set is stated to be obviously orthogonal; doubtless, if one spent half an hour trying to understand the definition, one could, within another half-hour, verify the orthogonality. On p. 40, however, the authors demonstrate in complete detail the orthogonal properties of $\sin nx$, $\cos nx$. I could wish that they had regarded this as obvious and demonstrated the preceding proposition.

Many passages in this book seem to cry aloud "Give us references, or give us proofs!" But they cry in vain. I cannot help suspecting that the peculiar system of reference which the authors use is responsible for this pathetic spectacle. In the margin, against any proposition, formula or paragraph to which they expect to refer, occurs a symbol of the form $[\alpha\beta\gamma]$ where α, β, γ are independent variables defined in the field of the first nine natural numbers; α denotes the chapter, β the section, γ the ordinal number of the reference in the section. At the end of the book is a list of some of these symbols, and against each a number, indicating the book or paper in the bibliography where further information may be obtained. "Auf diese Weise", say the authors, "haben wir Fusznoten entbehren können."

In taking leave of this book, let me thank the authors for a few weeks' interesting, if strenuous, reading matter. I look forward to seeing a second edition, with the misprints corrected, the obscurities elucidated and the language embellished. S. V.

Modern Pure Solid Geometry. By N. ALTSHILLER-COURT. Pp. x, 311. $3.90. 1935. (Macmillan Company, New York)

It has been a pleasure to read this work. As the author justly claims, there is a real need for textbooks dealing with Solid Geometry on the same lines as the various "sequels" to Euclid. Dr. Altshiller-Court has collected propositions from highways and byways, arranged them neatly and logically and, in the event, has produced an attractive volume which will go far to supply this need. The book is not encyclopaedic and individual readers will perhaps query certain omissions (the present reviewer, for example, is of opinion that some account of vectors should be given in connection with the tetrahedron). Such matters are, however, largely questions of taste and judgment and an author, especially one who has actually taught the subject in a university course, may be allowed to have his own views.

As hinted above, the arrangement of the book is modelled on that of Modern Plane Geometries; the tetrahedron takes the place of the triangle and the sphere that of the circle. Thus orthogonal spheres, inversion, spheres with a common radical plane and those with a common radical axis receive adequate treatment, and many problems are discussed, including the famous problem of Apollonius: "To draw a sphere to touch four given spheres." An interesting feature is the way in which analogies are brought out between theorems of plane geometry and those of space. It is mentioned, for instance, that to concurrent lines in a plane may correspond not only concurrent lines in space, but also generators of a hyperboloid; midpoints of sides of a triangle are analogous to midpoints of edges of a tetrahedron and also to the circumcentres of the faces. A chapter on the oblique cone with circular base suggests this surface as an analogue to the triangle in addition to the more obvious tetrahedron and skew quadrilateral. Such hints are helpful in the search for new theorems in geometry. Would it be true to say that *every* theorem in plane geometry can be extended to three dimensions?

The chapters on the tetrahedron include a thorough discussion of escribed spheres. By considering the signs of perpendiculars from a point to the faces,

it is clear that there should be eight spheres touching all four faces. Taking away the inscribed sphere, we are left with the curious number of seven escribed spheres, disposed around a figure with four faces. It is a good, but strenuous, exercise in visualisation to " see " these spheres in relation to the tetrahedron. Actually their position depends on the relative areas of the faces ; there are always four spheres placed like the escribed circles of a triangle, but the remaining three vary, certain regions being mutually exclusive.

A large number of exercises and a full and interesting bibliography (in which, however, one regrets to find no references to the *Mathematical Gazette*) add to the value of a pleasant and instructive textbook. May one point out a slight misprint in § 243 and the misspelling of Lord Kelvin's name as Thompson in the bibliography ?

H. L.

Vorlesungen über Elementare Mechanik. By J. NIELSEN. Pp. x, 500. RM. 38 ; geb. 39.60. 1935. Die Grundlehren der Mathematischen Wissenschaften, Band XLIV. (Springer, Berlin)

This book is based on lectures delivered by Professor Nielsen at the Technical High School, Copenhagen, their translation into German being made the opportunity for some amplification. The use of the word " elementary " in the title is somewhat misleading, for such topics as three-dimensional statics and dynamics, Lagrange's equations, and potential theory are included.

The author's treatment of the subject is so different from that usually given in this country that it is difficult to see where this book will fit into a mathematical course. For example, before the reader is introduced in the second chapter to Lami's theorem or to Hooke's law he will have come across the principle of Virtual Work, and will have been expected to know the equation of the tangent plane to a surface. The lectures do, in fact, form a course of abstract mechanics suitable for the mature mathematician, and in them there is little contact with real problems. They are the antithesis of our course in mechanics for engineering students.

But because the book does not conform to the traditional lines of teaching in this country it is by no means to be condemned. The treatment is, for the most part, by vector methods, and some of these are very elegant. Professor Nielsen does not hesitate to use any mathematical tools that can be of service. Explanatory chapters on vectors and matrices, on vector systems, and on tensors are included.

The author, following the usual Continental practice, gives very few examples, either worked in the text or left as exercises for the reader. He explains that this deficiency is due to the need of keeping the book within reasonable limits. Teachers will agree with his statement that one of the chief difficulties of learners is the application of the general theory to concrete problems, and in this book the general theory itself is undoubtedly difficult.

Those who are already familiar with this subject as it is usually presented in English textbooks can learn much by studying this course of lectures.

R. O. S.

Methodische Einführung in die höhere Mathematik. By K. REINHARDT. Pp. iv, 270. Geb. RM. 14. 1934. (Teubner)

The author says that the aim of the book is to bridge the gap between " Schul- und Hochschulmathematik ". The book begins with a number of calculations of areas under curves by very detailed elementary methods applied to sums of rectangles which approximate to the area on one side only. Alternating with these chapters are some which develop carefully the properties of limits of sequences and of bounds of sets of numbers, and point out where such

properties have been used in the calculations of areas, which are, at present, assumed to exist. There are several points of interest in the discussions. One is the treatment of the area under the hyperbola $y=1/x$, which involves the use of logarithms to an arbitrary base as defined in elementary algebra, and introduces e as the common limit of the sequences

$$\left(1+\frac{1}{n}\right)^n, \quad \left(1+\frac{1}{n}\right)^{n+1}.$$

Properties of continuous functions are next explained, but the uniform continuity theorem is not included. This omission makes the proof of the existence of the Riemann integral for a continuous integrand seem complicated. Integration is defined only for continuous functions. The definition is first for positive integrands, next for negative integrands, and then it is extended to continuous functions with only a finite number of zeros. It is a pity that the removal of this restriction is relegated to the examples and that the later parts of the book consider only this special type of continuous functions. In addition to the ordinary Darboux and Riemann sums for approximating to $\int_a^b f(x)\,dx$, the author considers sums of the types

$$\sum_{n=1}^{\infty} M_n(x_n-x_{n-1}), \quad \sum_{n=1}^{\infty} m_n(x_n-x_{n-1}), \quad \sum_{n=1}^{\infty} f(\xi_n)(x_n-x_{n-1})$$

where (i) $x_0=a<x_1<x_2<\dots$, and $x_n\to b$.

(ii) M_n, m_n are the bounds of $f(x)$ in (x_{n-1}, x_n).

(iii) ξ_n is an arbitrary point in (x_{n-1}, x_n).

Approximative sums of the ordinary types and of these generalised types are also considered in relation to infinite integrals of the first and second kinds.

After a discussion of derivatives, their principal properties, and the relations between differentiation and integration, other limiting processes are introduced. The discussion of infinite series includes the proof of Maclaurin's and Taylor's theorems by repeated integration by parts, and a proof of the irrationality of e.

It is good to find something about continued fractions and one would have wished for more, as only one type is considered. This is the fraction

$$q=\frac{a_1}{b_1-}\,\frac{a_2}{b_2-}\,\frac{a_3}{b_3-}\cdots,$$

in which $a_n>0$, $b_n\geqslant a_n+1$. It is proved that (i) q converges, (ii) $0<q\leqslant 1$, and (iii) q is irrational if $b_n>a_n+1$ for all but a finite number of values of n. In this connection a beautiful formula, due to Euler, for $\tan x$ is proved:

$$\tan x=\frac{x}{1-}\,\frac{x^2}{3-}\,\frac{x^2}{5-}\,\frac{x^2}{7-}\cdots \quad (x\neq 0)$$

and applied to prove that π is irrational.

A chapter introductory to the theory of trigonometrical series is given; the results proved are that the system $1, \cos x, \cos 2x, \dots, \sin x, \sin 2x, \dots$ is complete for continuous functions, and that a function with a continuous second derivative is the sum of its Fourier series.

The remaining topic of the book is the fundamental theorem of algebra. This is proved in the form that any polynomial with real coefficients is expressible as a product of real linear and real quadratic factors.

Extensions of the subject matter are indicated in some of the examples at

the end of each chapter and an appendix gives historical notes on the various chapters.

The book succeeds in effecting a gradual and interesting introduction of the ideas of accurate analysis and an introduction to some of its branches. If a wish arises that some of the earlier calculations had been omitted in order to carry some of the later work a little further, that is balanced by feeling that perhaps that would have introduced the difficulties of the more formal side of the work too rapidly to those for whom the book is intended. R. C.

Einführung in die Vektor- und Tensor-Rechnung, unter besonderer Berücksichtigung ihrer physikalischen Bedeutung. By H. SCHMIDT. Pp. v, 125. RM. 5.80. 1935. (Jänecke, Leipzig)

The author of this little book considers that the subject of vector analysis has usually been presented in too loose a form. Owing to the supposed necessity of appealing almost exclusively to physicists and engineers, the arguments have been based upon geometrical intuition rather than exact reasoning. Dr. Schmidt believes that he has made the subject clearer to the young mathematician by using exact logical considerations, drawn from algebra and analysis. Perhaps this may be so for Germans, but it is doubtful if it is true for Englishmen, who, as a race, are not passionately addicted to purely abstract reasoning. As Klein remarked, "Mathematical teaching is a function of two variables, the subject and the pupil." Dr. Schmidt, in his preface, has some interesting remarks on the unpopularity of mathematics in German schools, apparently due very largely to the manner in which the subject is presented. His own remedy (a sound one, in English opinion) is to connect the subject with its applications to physics and engineering. Thus while doubts arise about his first chapter (vector algebra with the geometry reduced to a minimum), his second chapter (vector analysis with applications to kinematics and hydrodynamics) appears much more acceptable. The third and last chapter opens brilliantly, with an exposition of tensors as arising naturally in the theory of elasticity. Unfortunately it is very short (consisting of only twenty-one pages) and abandons the subject before it has been developed far enough for the usual applications to relativity or other branches of physics.

The printing is clear if rather small (especially in the preface) and the diagrams are well executed. H. T. H. P.

Probability and Random Errors. By W. N. BOND. Pp. viii, 141. 10s. 6d. 1935. (Arnold)

Dr. Bond is Lecturer in Physics in the University of Reading, and in the course of his work there has found a need for a book on measurements of observations for students of physics and chemistry. He claims some originality of treatment, and a summary of the chapter headings may perhaps indicate, in some measure, the justice of his claim. Chapters I and II deal with problems on probability, chapters III and IV on errors, chapter V on combination of observations, VI on correlation, VII on curve fitting and VIII on periodic curves. There are also some miscellaneous examples and two appendices (one of formulae) and an index. There are very few references; those that are given are chiefly for details of experimental data in physics and chemistry to illustrate his points. The only references to writers on statistical theory are to Whittaker and Robinson's *Calculus of Observations* and to a paper by Harold Jeffreys in *Proc. Royal Soc.* This is unfortunate, for much valuable work on small samples has recently been done with which the author appears unfamiliar. In fact he fails to have realised the limitations of the fundamental assumptions that he claims to wish to discuss when the samples are small.

The work of Fisher, Wishart and Yates, *e.g.*, in this topic has, it is true, been done largely in the sphere of agricultural and biological experimental work, but the principles are wide reaching and some reference should surely be incorporated in a book that professes to be up to date and that essentially deals with small samples (*i.e.*, experiments of the order of twelve in number or even fewer). In dealing with "readings on a scale" there is no reference to Yule's important paper; in dealing with survival no reference to Brownlee. It is surely not correct to refer to the random walk with deviations to left and right as a one-dimensional problem, and it is inconvenient to find μ used as the symbol for the root mean square error. We notice the following misprints:

P. 13A. Solidus is twice omitted.
P. 105. For "enabled" read "enables".
P. 119, l. 5 from bottom. In the second bracket, read λ_4.

No doubt in Dr. Bond's own hands the book will be invaluable to his students; for other students who need a textbook to the subject the teacher will find much of use, and with his careful guidance the student should find much of value in it and from a careful reading of it should come away with a useful knowledge of some of the difficulties of measurement, and of the methods of dealing with them to obtain sound conclusions. F. S.

The History of Mathematical Teaching in Scotland to the end of the Eighteenth Century. By DUNCAN K. WILSON. Pp. viii, 99. 5s. 1935. (University of London)

This work is the result of a detailed study of Scottish school books in mathematics, and is one of the publications of the Scottish Council for Research in Education. Although its scope is the teaching of mathematics in Scotland from earliest times to 1800, yet the greater part of the book is concerned with conditions in the eighteenth century.

It is almost impossible to write a book in which the teaching of a subject is divorced from its development, and Dr. Wilson evidently recognises this for he has attempted to do both in ninety-nine pages! In consequence the book is the merest outline of what could have been made interesting, valuable and even surprising. It leaves two impressions on the reader, both negative; the first is a bad omission by the author, and the second a serious neglect by his native land. In the course of his researches the author is naturally forced to give some account of the development of arithmetic in the period considered, and in doing so mentions certain Scots who helped to establish the decimal notation. He omits the name of Napier in this connection; apparently he has not read the *Constructio*, the *Descriptio* or *De Arte Logistica*—they do not appear in his bibliography—and in them Napier gives a decimal notation very little different from our own. Anyone writing on the development of mathematics in Scotland should read at least all the works of Napier, James Gregory, David Gregory and Maclaurin, and a Scot writing on behalf of a Scottish research council cannot be pardoned for not doing so. Napier's "rods" and logarithms are mentioned, as well as the great impetus that was given to the teaching of mathematics at the universities by David Gregory and Maclaurin, but James Gregory has been forgotten. The second impression is that logarithms were not taught in Scottish schools for about 150 years after their discovery, while the use of the "rods" was almost universally known.

From Dr. Wilson's researches it appears that mathematics was of little account before 1700—in fact the most ambitious curriculum offered in this period was at the monastery school of Kinloss, an establishment which disappeared at the Reformation. The grammar schools for many years refused to

teach even arithmetic, and so "academies" were started in many towns. A boy who desired to enter the Army, Navy or Commerce enrolled at an academy after completing his course at a grammar school, and was there taught a little elementary mathematics. In the course of time the grammar schools allowed pupils to visit the academies for instruction perhaps once a week, but such visits were made as difficult as possible by arranging them at inconvenient hours. It was not until about the beginning of the nineteenth century that the grammar schools appointed teachers of mathematics. From about 1640 some arithmetic was taught in the schools that fed the grammar schools, but the quality may be gauged from the fact that boys entered these latter institutions at eight or nine years of age. Also the fare provided was such that finger-reckoning and the use of Napier's "rods" were included in every arithmetic curriculum until about 1720. At the universities little or no attention was paid to mathematics until the seventeenth century, when the subject was made compulsory, but even then the little Euclid in the syllabus was taught with very few proofs of the theorems.

In the eighteenth century arithmetic came into its own mainly because of the needs of commerce, and the curriculum also included trigonometry, which was required for gauging, surveying and navigation; a little algebra was taught, enough to enable pupils to solve equations, this being considered the chief use of the subject. By the end of the century algebra included permutations, combinations, probabilities, coordinate geometry, graphs, "fluxions and indirect fluxions."

On the whole the book is disappointing, probably because so little has been made of so much. Minor details are given prominence while the major are omitted, and it seems a great pity that the result is out of all proportion to the effort that must have been involved in its compilation. ALEX. INGLIS.

Le Livre d'Algèbre du Baccalauréat. By L. LONG. Pp. 210. 20 fr. 1935. (Gauthier-Villars)

This book is in three sections, the first of which deals with equations and problems of the second degree. Here the "theory of quadratics" is developed with great elaboration. Thus it is shown that—$f(x)$ standing for ax^2+bx+c—the equation $f(x)=0$ has two roots lying between the two given numbers α and β if the following conditions are satisfied:

$$\text{(i)}\ af(\alpha).(b^2-4ac)>0;$$

$$\text{(ii)}\ af(\beta).(b^2-4ac)>0;$$

$$\text{(iii)}\ (2a\alpha+b)(2a\beta+b)<0:$$

either (i) or (ii) can be replaced by $f(\alpha)f(\beta)>0$. As part of an elementary study this is somewhat severe discipline; but the concluding chapter of this section deals with an interesting set of geometrical problems in which many of the earlier algebraic conditions appear as visible requirements of actual lines and circles. The second section of the book deals with variation of the functions ax^2+bx+c and $(ax+b)/(a'x+b')$. Here, of course, coordinates are introduced and the parabola and the hyperbola are named. The last section deals with arithmetical and geometrical progressions and with common logarithms. This apparently concludes the programme of the first part of the *baccalauréat*. "Imaginary" numbers are not mentioned. A very important part of the book is its fine collection of examples and problems (some of them annotated); they occupy nearly a third of its two hundred pages. Those who teach algebra in the upper forms of schools will find the book very interesting and very useful. T. M. A. C.

Infra-red and Raman Spectra. By G. B. B. M. SUTHERLAND. Pp. viii, 112. 2s. 6d. 1935. Monographs on physical subjects. (Methuen)

Those responsible for Methuen's Physical Monographs are fortunate in finding an author so expert on both the experimental and theoretical sides of the subject to make this recent addition to their series. In this book Dr. Sutherland confines himself to the by no means narrow field of application of infra-red and Raman spectra to the study of molecular structure. This is, of course, one of the most important applications of modern atomic physics, being, in fact, the first step in the process of basing the physics of matter in bulk on the physics of the atom. The book is not, however, intended to appeal directly to the mathematician, though mathematical physicists interested in this field will welcome it as a most illuminating survey of the utilisation of their work. Not the least striking feature is the way in which the book brings home to the reader the enormous rate at which a branch of science, which was scarcely known ten years ago, can grow under modern conditions of research.

W. H. McC.

Thermionic Emission. By T. J. JONES. Pp. viii, 108. 3s. 6d. 1936. (Methuen)

This little volume is one of a series of monographs on physical subjects. It can be regarded as a companion to the well-known earlier volume *Thermionic Vacuum Tubes*, in which Professor E. V. Appleton has treated the applications of thermionic emission in radio and kindred fields. The author commences with a short historical survey and traces the developments between 1900 and 1913. He then deals with the vast amount of experimental and theoretical work which has been done during the past two decades. Sufficient theory is introduced to enable the reader to follow the general lines of development. The " shot " and " flicker " effects, which are so important in valve amplifier design, are explained lucidly. The beneficial effect of thorium in stimulating the electron emission from tungsten and atomic emitters in general is considered fully, whilst filaments coated with oxides of barium and the like are treated at length. These oxide-coated filaments are of paramount importance in radio receiving valves. The text closes with a chapter on the thermal emission of positive ions, and this is followed by an index and an extensive bibliography. The subject in its ramifications is presented in a concise, well-ordered manner, and the book can be recommended to anyone who wishes to study and acquaint himself with the latest developments of thermionic emission.

N. W. MCLACHLAN.

Ladders, or the Common Sense of School Mathematics. By E. ALLAN PRICE. Pp. viii, 84. 2s. 6d. 1935. (Oxford)

This book contains a summary of the work for the elementary and additional mathematics of a school certificate examination. Like the author's pamphlet *Logical Sequence in Plane Geometry* it may help towards intelligent revision.

It has the laudable aims of emphasising logical continuity and helping the student to take stock of his knowledge. He is to look both up and down the ladder.

Fearful lest the student should fail to see the wood for the trees the author has reduced the trees to neatly arranged piles of timber, and this does not improve the landscape.

The section on coordinate geometry begins with the discouraging remark that " algebraic geometry of a simple kind is especially valuable as an exercise in the manipulation of equations ", and it ends with a glance up a very Cartesian ladder.

An unintelligent use of the book might degenerate into cram. The author is probably aware of this danger when, in the last sentence of the preface, he advises the doubtful reader to read the preface again. The obedient sceptic will remain in this vicious circle. A. R.

Education for Citizenship in Secondary Schools. By various Authors, with a foreword by the Right Hon. OLIVER STANLEY, M.P. Pp. x, 267. 4s. 6d. 1936. (Oxford)

The first part of this volume is an interesting plea for the inclusion in the secondary school course of special teaching for Citizenship; then follows the larger part of the book, which shows how this teaching should be introduced into the teaching of History, Geography, Economics, Politics, English, Classics, Modern Languages, Mathematics and Science. Sir Percy Nunn deals with Mathematics and gives an outline of his well-known syllabus for "The Arithmetic of Citizenship" under the headings: Rates and Taxes, Private and Public Loans, Types and Methods of Insurance, The Statistical Sense, Mathematical Correlation, Banking and Currency. A. W. S.

Higher School Geometry. By L. Crosland. Pp. xiv, 320. 6s. 1935. (Macmillan)

This book is described by its author as an attempt to put between two covers a course in geometry suited to the needs of post-certificate students taking a two-year course in mathematics and science in preparation for a Higher Certificate examination. Such students need a slight amount of advanced geometry of the triangle, some solid geometry and a good deal of analytical geometry. The selection of textbooks is not easy. Most teachers find it necessary to issue three separate volumes on pure, solid and analytical geometry, but the ever-increasing difficulty of Higher Certificate papers makes it essential to select those portions of the volumes which are likely to appear. This is tiresome for the teacher and bewildering for the pupil—who much prefers to steer a clear course through a single book. Mr. Crosland's volume will receive a cordial welcome from all who have had this experience of selection, whether as teachers or as taught. It is an honest attempt to deal with a difficult situation, and is obviously the work of a teacher who has had much experience in preparing candidates for Higher School examinations. As one instance of this we may mention that the chapters on analytical geometry are arranged to permit of a preliminary reading based on numerical examples. Generally speaking the methods used are straightforward and planned to convince the average, rather than to enlighten the mathematically gifted, pupil. Thus, to find the perpendicular distance of a point from a line a method is used which depends on solving simultaneously the equations of the line and a perpendicular to it through the point. Calculus is used for obtaining the equations of tangents and normals. Each chapter is refreshingly compact (from the point of view of examination preparation) and no doubt many pupils could work through the book quite happily by themselves. The book opens with analytical geometry; there are chapters on the straight line, loci, the circle and conics. There are two chapters on conics; the first deals with their standard equations, the second with tangents, diameters, pole and polar. A chapter on parameters concludes this section.

The next three chapters are unusual. The first deals with the parabola; the analytical geometry of the curve is rapidly revised and extended, and then follows a discussion by pure geometry—"geometrical conics" in fact. A similar method is applied to the ellipse and the hyperbola. Finally the pure geometry of the triangle and the rudiments of solid geometry are treated in two concise and straightforward chapters. The book is well provided with examples,

revision papers, worked examples and an index. It is very well printed and gives the impression of thoroughness and substance. Occasionally there are flaws. The numerical work seems a little overdone on p. 54 and the definition of an asymptote on p. 103 is questionable. More serious criticisms might be made, but these would really be criticisms of the Higher School syllabus rather than criticisms of Mr. Crosland's attempt to cope with that syllabus. It does seem unfortunate, however, that there should be so much attention given to the pure and analytical geometry of the conics without any reference to their projective properties (apart from those deduced by orthogonal projection). One may also regret the absence of curves other than the conics. Even polar coordinates appear less for their own sake than for their usefulness in deducing formulae for Cartesian coordinates. Possibly the time is ripe for a reconsideration of Higher School mathematics, and indeed we understand that two university examining boards are at present deliberating upon the scope and purpose of the whole examination. As the mathematical syllabuses stand at present Mr. Crosland's book offers an adequate preparation, but we hope that pupils will at least have access to such other works as Forder's *Higher Geometry* or Lines' *Solid Geometry*, so that the mathematical significance of Higher Certificate work may be brought home to them.

C. T. DALTRY.

Century Sum Books, 5A and B. By A. WISDOM. Pp. 64 each. 11d. each limp cloth ; 9d. each paper. 1935. (University of London Press)

These books each contain 1100 sums for children of eleven intended as "a thorough revision and deepening of the work of the previous four books". The sections revised include weights and measures, decimals, fractions, percentages, mensuration of rectangles. Both books deal with much the same work but the "A" book goes more rapidly and uses harder examples. With the exception of a few routine sums, all the questions set in the two books are different. In preparing the books Mr. Wisdom has had in mind the needs of children who are preparing for a preliminary scholarship examination or who wish to revise rapidly the work of a Junior School course. His books are so good that we hope they will be considered by all teachers of eleven-year-olds. Their arrangement is simple and efficient. Each topic is revised on a right hand page; at the head is a worked example, with rules where these are necessary. Below are two exercises on the topic. The opposite page contains a revision test—a set of mental questions and a set of written questions.

Mr. Wisdom's previous books on "Arithmetical Dictation" were original and excellent. They completely fulfilled the purpose with which they were written. The same should be true of the present books. C. T. DALTRY.

The London Arithmetics. By P. B. BALLARD and J. BROWN. Second series. Pupil's books, I, II, III. Pp. 80 each. Paper, 10d.; limp cloth, 1s. each. Teacher's book, III. Pp. 88. 2s. 3d. 1935. (University of London Press)

These books are attractively produced and strike an original note, for their compilers have introduced such pleasing illustrations that the dull appearance of the usual junior arithmetic is enhanced past all knowledge. The wording of the sums is also exceptionally interesting.

For general class-teaching the books would need augmenting, for although the examples are well graded with some easy and many revision exercises, the average and slightly sub-average pupil would need more easy examples. The clever child, however, would find these books most stimulating.

E. G. SMART.

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